DOCUMENT RESUME

ED 206 869 CE 029 978

TITLE	Enhanced Work Projects--The Supported Work Approach for Youth. Youth Work Experience. Youth Knowledge Development Report 7.3.
INSTITUTION	Manpower Demonstration Research Corp., New York, N.Y.
SPONS AGENCY	Employment and Training Administration (DOL), Washington, D.C. Office of Youth Programs.
PUB DATE	May 80
NOTE	448p.; Some tables will not reproduce well due to small print. For related documents see note of CE 029 968.
AVAILABLE FROM	Superintendent of Documents, U.S. Government Printing Office, Washington, DC 20402 (Stock No. 029-014-00171-6, $8.50).
EDRS PRICE	MF01/PC18 Plus Postage.
DESCRIPTORS	Adolescents; Adults; *Cost Effectiveness; *Delinquency; *Demonstration Programs; *Dropout Programs; Dropouts; Employment Programs; Federal Programs; Minority Groups; *Program Effectiveness; Unemployment; Work Experience Programs; *Youth Employment
IDENTIFIERS	*Supported Work Programs; Youth Employment and Demonstration Projects Act

ABSTRACT

This volume is one of the products of the knowledge development effort implemented under the mandate of the Youth Employment and Demonstration Projects Act of 1977. The report focuses on the young school drop-out portion only of the supported work experiment, which also included offenders, ex-addicts, and welfare recipients. The goal of supported work is to aid groups of people with well-established employment difficulties to obtain and keep a regular job; in addition, it aims to reduce welfare dependence, drug use, and criminal activity. Between April 1975 and July 1977, five of the demonstration sites--Atlanta, Hartford, Jersey City, New York, and Philadelphia--participated in the supported work evaluation sample. They enrolled 1,244 unemployed 17- to 20-year-old high school dropouts, offering a randomly selected subset of the enrollees employment in a supportive environment for up to 12 or 18 months, and assistance in finding other employment as their program eligibility ended. Data were gathered for 18 months for 861 youths, although only 153 youths who enrolled in the sample prior to April 1976 completed an interview at 36 months. Thus, longer-term impacts of supported work are uncertain. The general conclusions from this study are that the availability of a supported work job had short-run impacts on employment and, consequently, on dependence on public assistance, but that the program does not appear to have mitigated the long-run employment problems of these youths. Furthermore, the project had no impact on youths' drug use or their participation in crime. Therefore, supported work does not appear to be well-suited to ameliorating the employment problems of young dropouts; and the net cost of the program is high. (KC)

YOUTH KNOWLEDGE DEVELOPMENT REPORT

YOUTH WORK EXPERIENCE
Enhanced Work Projects—The Supported Work Approach for Youth

ED206869

May 1980

U.S. DEPARTMENT OF EDUCATION
NATIONAL INSTITUTE OF EDUCATION
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)
☑ This document has been reproduced as received from the person or organization originating it.
☐ Minor changes have been made to improve reproduction quality.
● Points of view or opinions stated in this document do not necessarily represent official NIE position or policy.

YOUTH KNOWLEDGE DEVELOPMENT REPORT 7.3

ENHANCED WORK PROJECTS--
THE SUPPORTED WORK APPROACH FOR YOUTH

MANPOWER DEMONSTRATION
RESEARCH CORPORATION

For sale by the Superintendent of Documents, U.S. Government Printing Office
Washington, D.C. 20402

OVERVIEW

Youth work experience in the public and nonprofit sectors offers a range of potential benefits:

First, there is direct output from the labor of participants. The extent of output depends on the skill and ability of participants, the emphasis on output vs. worksite learning, the degree of supervision and discipline, the inputs of capital and equipment, and the types of work which are being done.

Second, where work experience is targeted on youth from low-income families who would otherwise be unemployed, there is an income transfer effect as well as an equalization of employment probabilities. Employment problems and low family income are highly correlated, but the match is not exact. The stricter the income targeting standards the more likely that participants on the average will have serious problems but the more likely also that other youth with serious needs will be excluded. The more disadvantaged the participants, the more likely that the wages paid for work will exceed productivity and that there will be an indirect income transfer contained in the wage, and the more likely that the participants would have been otherwise unemployed increasing the net employment impact. Income transfers linked to work are presumably preferable to direct cash payments. Equalization of employment chances is a desirable goal, particularly where discrimination or other structural factors are a cause of the inequality.

Third, there are potential in-program benefits resulting from work experience. One theory is that the jobs serve as "aging vats" in which normal maturation occurs under favorable conditions, i.e., work keeps youth constructively occupied and helps them avoid the dislocations which might impede subsequent progress in the labor force. Jobs may teach about the world of work and its demands, with the result that participants gain in job holding and job seeking skills. The workplace can be structured as an environment where behavior modification occurs which would be reflected in increased self-esteem, dependability and the like. Work experience programs can be a sorting mechanism where disadvantaged youth who are normally excluded by employers because of background characteristics can prove they are dependable and productive and can acquire a resume and credentials. The work project may emphasize training in which case specific skill acquisition would occur. The work experience may be closely linked to education or outside training and thus used as an inducement for participation in and completion of other activities. Finally, the job may be structured as the first rung in an identified career ladder, providing access which would otherwise be unlikely.

Fourth, work experience may have a range of post-program effects. Where work is tied to permanent jobs and to entry tracks into career employment, access to higher wage, more stable or upwardly mobile jobs would be the outcome. If the work experience emphasizes behavior modification or world-of-work exposure, then former participants would presumably compete more effectively in the labor market and there would be an impact on labor force participation and employment chances, and perhaps some reduction in undesirable social behavior. If training and education are the focus, youth might be more inclined to return to or complete school and advanced training. If the jobs are merely aging vats, little change would occur for most participants although a few less would enter adulthood with criminal records or illegitimate children to the extent these are related to joblessness.

There is infinite variation in the possible elements of work experience programs and hence in the potential benefits. There are also tradeoffs. For instance, the most productive work settings are those employing youth who have the fewest problems and who will make the best workers. In such settings, it is also easiest to structure linkages and placement into permanent career jobs. Limited emphasis will be placed on basic skills training or behavior modification so there will be few expected in-program changes. Likewise, the income transfer and employment equalization impacts will be limited to the extent participants have high probabilities of finding work on their own. There is also less impact to the extent resources are used for supervision or for non-labor inputs which make the work more productive. At the other extreme, the jobs might place limited emphasis on output and much more on learning. They would be targeted to youth with severe problems and would have a high transfer effect. The in-program changes might be extensive but the measurable post-program outcomes slight, at least in the near term.

Clearly, then, youth work experience is not a standardized activity. The appropriate design depends on a number of variables such as whether the work is summer only, in-school, or year-round for out-of-school youths, whether it is directed to teenagers or young adults, and the level of needs of participants. Unfortunately, there has been very little progress in determining the tradeoffs between the various goals or the most appropriate designs for different circumstances.

The supported work demonstration represents the most rigorous experiment to date with the work experience approach. In 15 sites around the country, work experience projects were implemented and carefully studied. Persons enrolled in supported work were assigned to crews comprised of individuals like themselves, usually 10 or less, with a supervisor to serve as a foreman and counselor. Graduated responsiblity and stress was employed in work tasks, along with peer group interaction to help youth cope with problems and adjustments. The employment opportunities lasted up to one year in most cases. The work varied from construction to light manufacturing, spanning most of the tasks usually addressed by public and nonprofit sector work experience. Revenues from the sale of goods and services produced by the work crews were used to offset the costs. There was a heavy emphasis on management under these projects and careful research of the impacts.

There were four target groups among the 10,000 participants in supported work: welfare recipients, ex-addicts, ex-offenders and youth. The youth group was limited to drop-outs age 17 through 20, with 1 of every 2 required to have a record of delinquency or crime. There was no income targeting but the nature of the projects had this same effect. Thus, supported work sites are representative of better run projects for out-of-school youth, perhaps most like the Youth Community Conservation and Improvement Projects instituted under YEDPA. The impacts of supported work were carefully researched, and it is therefore suggestive of the effectiveness of youth work experience.

It is important, however, to stress some of the differences between supported work and other youth work experience programs. Supported work serves a somewhat older, and more frequently male and minority client group than YCCIP or SYEP; the participants are more disadvantaged as measured by arrests and education but less so when measured by receipt of cash assistance.

Percent	Supported Work	YCCIP	CETA II Youth Work Experience	SYEP
Male	84	74	51	51
Under age 20	84	98	87	95
Minority	94	55	49	69
High School Dropout	99	47	15	6
Criminal Record	57	6	3	3
Cash Assistance Recipients	26	31	35	39

With its emphasis on close supervision and careful management, supported work spends relatively less on participant wages and salaries. Income support amounted to only 47 percent of full costs compared to 78 percent under YCCIP in fiscal 1979, and 84 percent under SYEP in fiscal 1978.

Finally, supported work is a stable program. Under YCCIP, there is enormous turnover of projects from year to year, and the program itself only began in fiscal 1978. In contrast, supported work has operated since 1975 in some sites and the evaluation is based on costs which exclude start-up expenses. Supported work is certainly more stable than SYEP which is implemented anew each summer.

Keeping in mind these differences, supported work is suggestive of the impacts of at least one form of work experience for a youth target group very much needing help.

The impacts of supported work for youth can be summarized as follows:

- Output - It is estimated that the most likely alternative supplier could have provided the same output as the supported work projects for a price equal to 60 percent of the wages and fringes plus direct project costs or 45 percent of total supported work costs.

- Income Transfer - Youth participants received an average of $6,304 per year in wages and fringe benefits. Subtracting the estimated value of output per work hour net of project costs (i.e., the value added by youth labor), roughly two fifths or $2,500 was income transfer. This represented roughly a fifth of the total program cost.

o Employment Impact - Wages were slightly above the minimum and the participants were those who would be otherwise jobless, i.e., only a little over a third of the control group worked in the first six months after participants enrolled. In other words, the employment impact could not be increased much by better targeting or lower wages. Increasing the share of participant wages to total program costs, could, however, have increased the net effect. Almost all program hours were spent in work; hours could have been shortened and supplemented by unstipended training or counseling.

o In-Program Benefits - The most obvious in-program benefit is increased income. During the first 9 months after enrollment, the average earnings of experimentals was $338 per month compared to $126 per month for controls. The earnings for participants include some indirect income transfer, but other sources of direct income transfer were also available for participants and widely used by controls. The estimated income from various sources in the 9 months was $319 for participants compared to $176 for controls. During this 9 month period, the percentage of youths with any arrests was slightly higher for experimentals than controls - 17.1 vs. 16.8, after adjusting for differences. The percent with robbery arrests was slightly less - 3.1 vs. 3.4. The percent incarcerated was less, 8.9 vs. 11.6 and so were the weeks of incarceration, 1.04 vs. 1.62. In other words, there were some slight reductions in criminal activity or at least in incarceration for such activity. The percent reporting drug abuse other than marijuana or alcohol was lower - 11.3 vs. 14.2 percent but the percent using marijuana and alcohol was higher, 56.9 vs. 52.9 percent for any use of marijuana and 8.1 vs. 5.5 percent for daily use of alcohol. The impacts on development of world-of-work skills were not directly measured. However, 53 percent of participants left as a result of firing, incarceration, or resignation because of dissatisfaction with the job, which could not suggest gains unless these accrue from such negative outcomes.

o Post-Program Impacts - Supported work does not, apparently have significant transitional effects. Only 26 percent of youth trainees left for a job. In the 19 through 36 months after entry, when only a minuscule portion of participants remained in supported work, the average earnings per month of participants was $282 compared to $291 for controls even though the hours of average work per month were slightly higher for participants--77.7 vs. 74.2. It would appear that participation in supported work may positively impact on work propensities, but that wages of youth who have been continuously in the regular labor market increase with accumulated seniority and advancement to higher paying jobs. This is suggested by the fact that the experimental-control differential widens over the post-program period in favor of experimentals when assessing employment rates and average hours worked, even though the average earnings per month widens in favor of controls. The immediate post-program period results in higher crime and arrest rates for participants relative to controls, then there appears to be a reversal in the 19-27 month period. In terms of drug and alcohol use, there are minor differentials in favor of controls in the post-program period.

The benefit-cost methodology may be used to analyze these impacts and to determine whether the supported work investment is worthwhile to society. Under the most reasonable assumptions, the costs exceed benefits by a net present value of $1,465 per year; in other words, society gets less back in terms of output, crime and drug abuse reductions, increased tax payments, reduced transfer program administrative costs, and other benefits than it pays out. Direct transfer payments are $329 less during the 1-9 month period and $102 less in the 10-18 month period, although indirect transfers (wages paid in excess of productivity) unquestionably exceed this amount significantly. Transfers are not counted as a social benefit, nor is there any differentiation between transfers tied to work (i.e., part of wages) and those paid regardless of productivity. During the period of program participation, income of participants was roughly $1,344 higher than that of controls and the estimated transfers (direct and indirect) were approximately $3,000. Depending on the value one might associate with these transfers, it is possible to consider the expenditures justified. It does not appear, however, that under any reasonable assumptions the social benefits of supported work for youth exceed the social costs by a substantial margin if at all.

The implications for work experience as a program approach depend on a comparison of supported work with other types of work programs, and must be considered in light of the disadvantaged population served by supported work. No current work experience program has, as yet, been as rigorously evaluated as supported work. There are, however, some conceptual comparisons in assessing benefits and impacts:

- Output - Value of output studies conducted using the same methodology as the supported work evaluation suggest that supported work is almost typical in its output per hour worked, but has a lower output per dollar of program cost because of the extensive administrative costs. For instance, a study of the 1979 summer program estimated a return of $2.08 in production for every hour of participation. Given expenditures for training, administration and the like, it is estimated between 55 and 60 percent of the cost of SYEP is returned in social production; this compares with 45 percent under supported work. YACC returns close to its cost and other local CETA work programs have output in excess of SYEP. In other words, it is definitely possible to mount less ambitious administrative arrangements and to get more output per dollar of total cost.

- Income Transfer - Again because of the high overhead, the income transfer effect is limited. SYEP in 1978 paid out 84 percent of costs in wages and salaries; YCCIP paid out 78 percent; where in supported work these income support components represented only 47 percent. Further, since these other programs were income targeted, they probably reached a poorer population (although not necessarily one with greater employment problems).

- In-Program Benefits - There is not comparable evidence on in-program effects of other programs, but it is certainly possible to increase "enrichment," or to consciously tighten or loosen the individual performance requirements.

- Post-Program Benefits - Supported work did not put emphasis on placement and the service deliverers were relatively isolated from CETA prime sponsors. The placement rate into unsubsidized jobs for YCCIP in fiscal 1979 (still the start-up period) was 21 percent, the same as supported work. More elaborate enhanced work projects such as Ventures in Community Improvement have been able to get youth into relatively better paying jobs; in other words, it is likely that post-program employment benefits exceed or can exceed those realized by supported work.

Supported work deals with youth who have severe problems. Its results may have been different with a less disadvantaged clientele. On the other hand, it may also represent the "wrong medicine" for such a group. The supported work clientele most nearly parallels that in Job Corps. The benefits and costs of Job Corps have been evaluated by much the same methodology, with a finding that benefits substantially exceed costs. Perhaps youth of this type benefit most from being removed from their environment and being provided with education and training rather than just work.

Another option is to increase the human resource development components as well as the transition services provided by supported work. The supported work demonstration has been extended with YEDPA funding to test the relative effectiveness of such enrichments. Further, there will be a long range follow-up of youth participants and controls from the first phase of supported work in order to determine whether the upward trend in employment for experimentals relative to controls in the post-program period continues or deteriorates, and whether the former participants catch up to the earnings of controls as a result of sustained labor force participation.

There are also two theoretical issues which will require more investigation. First, both "displacement" and "vacuum" effects occur, i.e., some of the publicly funded jobs displace those otherwise funded and workers who would fill them, while the net jobs created reduce competition and increase employment probabilities for non-participants. The extra jobs created by supported work certainly had little impact on controls, but presumably the job expansion effort would at least equal the foregone earnings of participants and would at most equal the full wage bill. In other words, the employment chances of all non-participants would increase as the competition for participants declined. This impact would be offset to the extent the jobs represented substitution for otherwise funded employment. The way supported work was operated outside regular employment structures and with the hardest core participants would suggest that the rate of substitution was substantially below that of public service employment as regularly operated under CETA, but that the vacuum effect was small. Both effects need to be studied.

A second issue is that of transition probabilities. The baseline for measuring post-program impacts under supported work was the comparative experience of experimentals and controls. Without assistance, a portion of youth will be expected to get jobs in any time period. Alternatively, a period of nonemployment between jobs can be justified by the job search theory or explained by a youth preference for intermittancy in labor force participation. If participants were compared to a simulated comparison group utilizing the positive transition probabilities of the control group since the beginning point, i.e., participants without jobs after leaving supported work were assumed to have the same employment chances as controls who were all initially without jobs, the relative benefits in the post-program period would appear greater and the participants would probably substantially exceed the simulated comparison group. In other words, if a job merely keeps a youth occupied, when this job is over he or she will likely be worse off relative to a group which had been in the regular labor force for some time, but better off than if the job had not been available. Given the volatile nature of youth labor force participation and the unemployment frequency associated with transition, a comparison group selected at the point of exit might be preferable to one selected at the point of entrance. Immediate post-program comparisons understate the impacts of work experience programs.

This report on supported work includes a detailed assessment of impacts and a benefit-cost analysis. It focuses on the youth portion only of the supported work experiment, which also included offenders, ex-addicts and welfare recipients. There were significant differences in impacts for these different clienteles and the full reports from the Manpower Demonstration Resource Corporation should be reviewed to pinpoint these differences. These reports include the Second Annual Report on the National Supported Work Demonstration and Summary and Findings of the National Supported Work Demonstration.

This volume is one of the products of the "knowledge development" effort implemented under the mandate of the Youth Employment and Demonstration Projects Act of 1977. The knowledge development effort consists of hundreds of separate research, evaluation and demonstration activities which will result in literally thousands of written products. The activities have been structured from the outset so that each is self-standing but also interrelated with a host of other activities. The framework is presented in A Knowledge Development Plan for the Youth Employment and Demonstration Projects Act of 1977, A Knowledge Development Plan for the Youth Initiatives Fiscal 1979 and Completing the Youth Agenda: A Plan for Knowledge Development, Dissemination and Application for Fiscal 1980.

Information is available or will be coming available from these various knowledge development efforts to help resolve an almost limitless array of issues. However, policy and practical application will usually require integration and synthesis from a wide range of products. A major shortcoming of past research, evaluation and demonstration activities has been the failure to organize and disseminate the products adequately to assure the full exploitation of the findings. The magnitude and structure of the youth knowledge development effort puts a premium on structured analysis and wide dissemination.

As part of its knowledge development mandate, therefore, the Office of Youth Programs of the Department of Labor will organize, publish and disseminate the written products of all major research, evaluation and demonstration activities supported directly by or mounted in conjunction with OYP knowledge development efforts. Some of the same products may also be published and disseminated through other channels, but they will be included in the structured series of Youth Knowledge Development Reports in order to facilitate access and integration.

The Youth Knowledge Development Reports, of which this is one, are divided into twelve broad categories:

1. Knowledge Development Framework: The products in this category are concerned with the structure of knowledge development activities, the assessment methodologies which are employed, the measurement instruments and their validation, the translation of knowledge into policy, and the strategy for dissemination of findings.

2. Research on Youth Employment and Employability Development: The products in this category represent analyses of existing data, presentation of findings from new data sources, special studies of dimensions of youth labor market problems, and policy issue assessments.

3. Program Evaluations: The products in this category include impact, process and benefit-cost evaluations of youth programs including the Summer Youth Employment Program, Job Corps, the Young Adult Conservation Corps, Youth Employment and Training Programs, Youth Community Conservation and Improvement Projects, and the Targeted Jobs Tax Credit.

4. Service and Participant Mix: The evaluations and demonstrations summarized in this category concern the matching of different types of youth with different service combinations. This involves experiments with work vs. work plus remediation vs. straight remediation as treatment options. It also includes attempts to mix disadvantaged and more affluent participants, as well as youth with older workers.

5. Education and Training Approaches: The products in this category present the findings of structured experiments to test the impact and effectiveness of various education and vocational training approaches including specific education methodologies for the disadvantaged, alternative education approaches and advanced career training.

6. Pre-Employment and Transition Services: The products in this category present the findings of structured experiments to test the impact and effectiveness of school-to-work transition activities, vocational exploration, job-search assistance and other efforts to better prepare youth for labor market success.

7. Youth Work Experience: The products in this category address the organization of work activities, their output, productive roles for youth, and the impacts of various employment approaches.

8. Implementation Issues: This category includes cross-cutting analyses of the practical lessons concerning "how-to-do-it." Issues such as learning curves, replication processes and programmatic "batting averages" will be addressed under this category, as well as the comparative advantages of alternative delivery agents.

9. Design and Organizational Alternatives: The products in this category represent assessments of demonstrations of alternative program and delivery arrangements such as consolidation, year-round preparation for summer programs, the use of incentives, and multi-year tracking of individuals.

10. Special Needs Groups: The products in this category present findings on the special problems of and the programmatic adaptations needed for significant segments including minorities, young mothers, troubled youth, Indochinese refugees, and the handicapped.

11. Innovative Approaches: The products in this category present the findings of those activities designed to explore new approaches. The subjects covered include the Youth Incentive Entitlement Pilot Projects, private sector initiatives, the national youth service experiment, and energy initiatives in weatherization, low-head hydroelectric dam restoration, windpower, and the like.

12. Institutional Linkages: The products in this category include studies of institutional arrangements and linkages as well as assessments of demonstration activities to encourage such linkages with education, volunteer groups, drug abuse, and other youth serving agencies.

In each of these knowledge development categories, there will be a range of discrete demonstration, research and evaluation activities focused on different policy, program and analytical issues. In turn, each discrete knowledge development project may have a series of written products addressed to different dimensions of the issue. For instance, all experimental demonstration projects have both process and impact evaluations, frequently undertaken by different evaluation agents. Findings will be published as they become available so that there will usually be a series of reports as evidence accumulates. To organize these products, each publication is classified in one of the twelve broad knowledge development categories, described in terms of the more specific issue, activity or cluster of activities to which it is addressed, with an identifier of the product and what it represents relative to other products in the demonstrations. Hence, the multiple products under a knowledge development activity are closely interrelated and the activites in each broad cluster have significant interconnections.

This volume should be assessed in conjunction with other studies of work projects in the "youth work experience" category, particularly Enhanced Work Projects--The Interim Findings From The Ventures In Community Improvement Demonstration. Because of the similarity of client groups and evaluation designs, The Lasting Impacts of Job Corps Participation also provides useful counterpoint.

Robert Taggart
Administrator
Office of Youth Programs

CONTENTS

Page

OVERVIEW i

THE IMPACTS OF SUPPORTED WORK ON YOUNG SCHOOL DROPOUTS 1

- I. Youth Unemployment and Supported Work 7
- II. The Evaluation Design and the Supported Work Programs Under Study 23
- III. Employment Related Impacts 59
- IV. Income, In-Kind Transfers, and Related Outcomes 107
- V. Impacts on Drug Use 119
- VI. Impacts on Criminal Behavior 135
- VII. Conclusion 151
- Appendix A - Supplementary Tables 159
- Appendix B - Assessing the Impact of Interview Nonresponse on Evaluation Results 191
- Appendix C - The Effects of Length of Time Spent in Supported Work on Program Impacts 219
- References 227

THE SUPPORTED WORK EVALUATION: FINAL BENEFIT-COST ANALYSIS 237

- I. The Supported Work Demonstration and its Evaluation 239
- II. Costs and Value of In-Program Output 255
- III. Benefits 291
- IV. Overall Results for the Youth Target Group 335
- V. Comparison to Other Benefit-Cost Studies of Employment and Training Programs 345
- VI. Conclusions 377
- References 386

THE IMPACTS OF SUPPORTED WORK ON YOUNG SCHOOL DROPOUTS

Rebecca Maynard

Mathematica Policy Research, Inc.

EXECUTIVE SUMMARY

The goal of Supported Work is to aid groups of people with well-established employment difficulties to obtain and keep a regular job. In addition to this major goal, other important objectives include reduction in such forms of behavior as welfare dependence, drug use, and criminal activity. In order to assess the program's success, a 5-year demonstration and research effort was undertaken. The four target groups that provided the focus for the demonstration are women who have been receiving welfare payments under the Aid to Families with Dependent Children (AFDC) program for substantial periods of time; ex-addicts who have recently been in drug treatment programs; ex-offenders who have recently been released from prison or jail; and young school dropouts, many of whom have records of delinquency.

This report focuses on the effects of Supported Work for young school dropouts. Between April 1975 and July 1977, five of the demonstration sites--Atlanta, Hartford, Jersey City, New York, and Philadelphia--enrolled in the Supported Work evaluation sample 1,244 unemployed 17- to 20-year-olds who had not completed high school, offering a randomly selected subset of the enrollees (experimentals) employment in a supportive environment for up to 12 or 18 months, depending on the site, and assistance in finding other employment as their program eligibility period neared its end. While interview data covering at least the first 18 months following enrollment are available for 861 youth, only 153 youth who enrolled in the sample prior to April 1976 completed a 36-month interview. Thus, conclusions concerning longer-term impacts of Supported Work are subject to considerable uncertainty.

The general conclusions from this study are that the availability of a Supported Work job had short-run impacts on employment and, consequently, on dependence on public assistance, but that the program does not appear to have met its central objective of mitigating the long-run employment problems of this disadvantaged segment of the youth population. Furthermore, there is little indication that Supported Work had either short- or long-run impacts on youths' drug use or their participation in crime. The key research findings on which these conclusions are based are summarized in Table 1.

> Employment and Earnings. The large employment gains of experimentals during the first few months after enrollment declined sharply as controls increased their employment substantially and experimentals left their Supported Work jobs (see Figure 1). By the start of the second year, when less than 20 percent of the experimentals were still in the

TABLE 1

SELECTED RESULTS FOR THE YOUTH SAMPLE

	Experimental Group Mean	Control Group Mean	Experimental-Control Differential
Employment and Earnings			
Percentage with Some Employment			
Months 1 - 9	97.6	53.3	44.3**
Months 10 - 18	68.0	60.2	7.8**
Months 19 - 27	61.5	61.0	0.5
Months 28 - 36	74.0	65.9	8.1
Average Hours Worked per Month			
Months 1 - 9	120.4	39.7	80.7**
Months 10 - 18	69.9	58.2	11.7**
Months 19 - 27	68.8	68.2	0.6
Months 28 - 36	88.6	81.4	7.2
Average Earnings per Month			
Months 1 - 9	$338.68	$125.48	$213.20**
Months 10 - 18	229.63	196.48	33.15*
Months 19 - 27	263.92	248.48	15.44
Months 28 - 36	301.94	336.33	-34.39
Income and Public Assistance			
Average Total Monthly Income			
Months 1 - 9	$391.05	$176.04	$215.01**
Months 10 - 18	287.39	265.44	21.95
Months 19 - 27	338.67	311.68	26.99
Months 28 - 36	353.47	408.01	-54.54
Average Monthly Welfare Income plus Food Stamp Bonuses			
Months 1 - 9	$ 31.59	$ 40.37	$ -8.79**
Months 10 - 18	32.32	48.44	-16.12**
Months 19 - 27	46.50	54.12	-7.62
Months 28 - 36	44.22	54.79	-10.57
Drug Use			
Percentage Who Used Any Drug Other Than Marijuana or Alcohol			
Months 1 - 9	11.3	14.2	-2.9
Months 10 - 18	10.3	10.2	0.3
Months 19 - 27	11.0	10.6	0.4
Months 28 - 36	16.8	11.0	5.8
Percentage Who Used Marijuana			
Months 1 - 9	56.9	52.4	4.5
Months 10 - 18	52.8	51.2	1.6
Months 19 - 27	57.9	57.6	0.3
Months 28 - 36	64.2	64.1	0.1
Percentage Who Used Alcohol Daily			
Months 1 - 9	8.1	5.5	2.6*
Months 10 - 18	11.2	9.3	1.9
Months 19 - 27	10.6	9.9	0.7
Months 28 - 36	7.3	8.9	-1.6
Indicators of Criminal Activities			
Percentage Arrested			
Months 1 - 9	17.1	16.8	0.3
Months 10 - 18	16.8	15.2	1.6
Months 19 - 27	10.8	14.0	-3.2
Months 28 - 36	10.3	16.7	6.4
Months 1 - 18	26.7	27.0	-0.3
Months 1 - 27	30.5	39.3	-8.8*
Average Number of Arrests			
Months 1 - 9	0.26	0.20	0.06
Months 10 - 18	0.21	0.18	0.03
Months 19 - 27	0.11	0.16	-0.05
Months 28 - 36	0.27	0.18	0.09

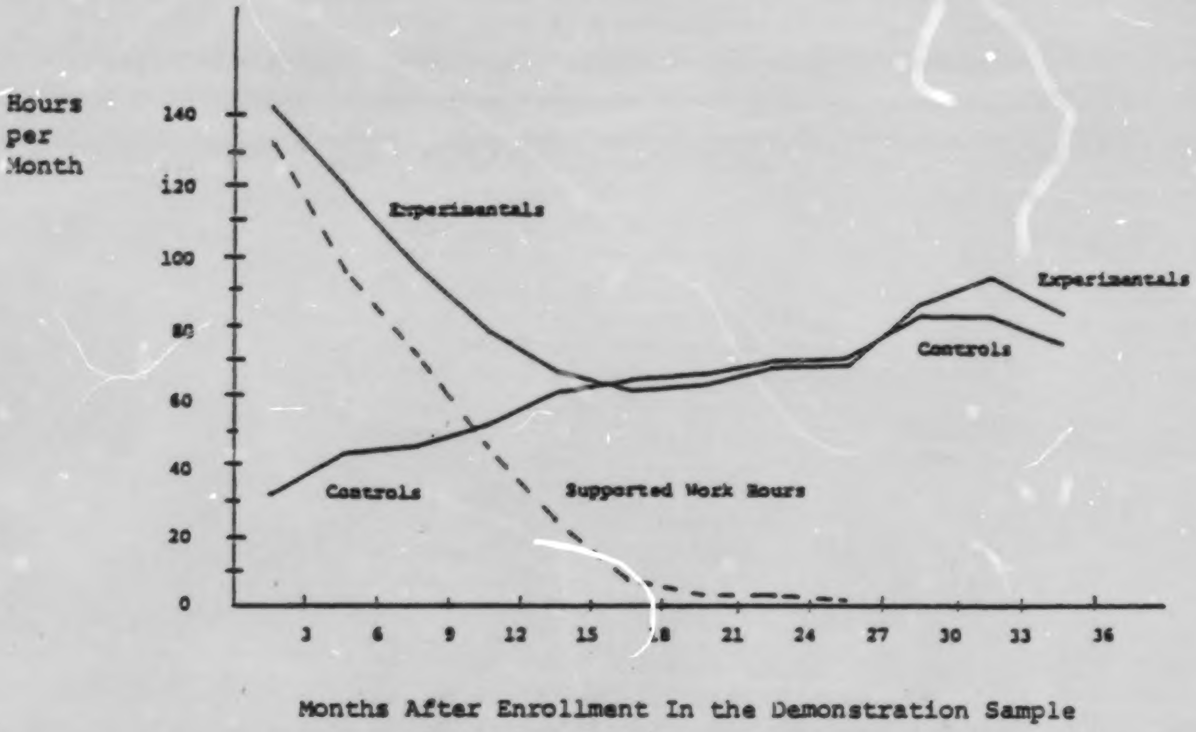
NOTE: These data are regression adjusted. The maximum sample size for results during months 1 to 18 is 861, that for results during months 19 to 27 is 513, and that for results during months 28 to 36 is 153. All figures pertain to the total sample.

*Statistically significant at the 10 percent level, two-tailed test.
**Statistically significant at the 5 percent level, two-tailed test.

FIGURE 1

TREND IN HOURS WORKED PER MONTH

YOUTH SAMPLE



NOTE: Experimental-control differentials are significantly different from zero only during the first 12 months following enrollment.

program, there was essentially no difference in the overall employment levels of the two groups. The re-appearing differential during the 31- to 36-month period is based on very few sample observations and is neither large nor statistically significant. Furthermore, the estimated earnings differential during this same period is negative implying that employed experimentals earned substantially lower wage rates than did employed controls. Perhaps the most noteworthy factor concerning these employment results is that both experimentals and controls exhibited a reasonably favorable pattern of employment; during months 19 to 36, between 61 and 74 percent of the sample reported employment during each 9-month period, and those with some employment worked the equivalent of about two-thirds time at wage rates averaging between $3.41 and $4.13 per hour.

Relatively more favorable patterns of effects were observed among those youth whose control group counterparts had low employment rates and levels--for example, the earliest enrollees in the demonstration who faced the poorest labor market conditions, those in New York, Hartford, and Philadelphia, and those who were younger than average. However, these findings generally were not statistically significant and so are only suggestive of a retargeting strategy to focus the program on a subset of youth who would be particularly likely to benefit from Supported Work.

Experimental youth did stay in Supported Work longer than controls stayed in nonprogram jobs (6.7 months versus 5.6 months for nonprogram jobs). However, longer tenure in Supported Work jobs has not been found to result in improvements in other dimensions of employment-related outcomes, such as employment rates, employment levels, or wage rates. Similarly, the Supported Work experience did not lead to substantially different types of jobs: among both experimentals and controls, two-thirds to three-quarters of their nonprogram jobs were in manufacturing, retail trade, and service industries, and they were mainly in clerical, service, and miscellaneous occupations.

Total Income and Welfare Dependence. Not surprisingly, given the employment results, Supported Work had short-term benefits for participants in substantially higher standards of living, with some small benefits to taxpayers in the form of reduced transfer payments. However, as experimentals left their Supported Work jobs, their total income once again approached that of the control group, which had risen substantially over time as a result of increasing earnings. Despite the upward trend in earnings, welfare payments to both experimentals and controls increased in absolute amount over time due to two factors: many youth had established their own households, and benefit levels rose over this period. However, the experimentals' average benefit level remained consistently about $10 to $15 per month below that of controls. A partial explanation for this persistent reduction in benefit levels among experimentals is their higher average unemployment compensation benefits during the 10- to 27-month period after enrollment.

Thus, while participants themselves benefited substantially from a Supported Work program, there was little compensating reduction in receipts from transfer programs. Over the first two years of the demonstration, participants experienced income gains averaging nearly $2,300 (about $1,900 of which they received during the time they were in Supported Work), a reduction in welfare and food stamp benefits of less than $300, and an increase in Unemployment Compensation benefits of about $130.

Drug Use. Supported Work had no overall impact on the prevalence of drug use. During each follow-up period, between 10 and 17 percent of both experimental and control youth used some drug other than marijuana or alcohol; 51 to 64 percent used marijuana, and 6 to 11 percent used alcohol every day or nearly every day. Furthermore, there is no strong evidence to suggest that the program had significantly different impacts for individuals with particular characteristics, nor is there a consistent relationship between experimentals' and controls' drug use and their employment status.

Criminal Activities. On average, the Supported Work employment opportunity had no significant impact on criminal behavior among sample youth, either during the time when experimentals were in their program jobs or subsequently. During each of the first two 9-month periods, about 17 percent of both experimentals and controls reported having been arrested and, among those arrested, the average number of arrests was between 1.2 and 1.5. Between 15 and 20 percent of these arrests were for robbery. During the 19- to 27-month period, a lower percentage of experimentals than controls reported the occurrence of an arrest (10 versus 14 percent), a result which, while not statistically significant, is related to consistently more favorable responses to Supported Work among those earliest enrollees who were followed for at least 27 months after enrollment. However, by the 28- to 36-month period after enrollment, there appears

> to be no further favorable impact of Supported Work on criminal activiites.
>
> There is no strong evidence to suggest that program effects on crime were substantially more favorable among identifiable subgroups of youth. As with the employment results, there are hints that, if the program affected involvement in crime at all, these effects were relatively greater among those who were younger. However, among other youth subgroups, the employment and crime results exhibited no consistent pattern.

On the whole, Supported Work does not appear to be particularly well-suited to mitigating the employment problems of those young school dropouts who are likely to apply for the program. As a consequence of the limited program impacts, the net social costs of Supported Work for youth are high. In a separate report, analyzing the benefits and costs of Supported Work, social costs are estimated to exceed benefits by an average of about $1,465 per participant youth. Thus in assessing whether or not Supported Work has a place among the federally sponsored programs attempting to deal with youth employment problems, it is important to weigh this net subsidy cost against the social objective of achieving relatively modest short-term employment and income gains.

CHAPTER I

YOUTH UNEMPLOYMENT AND SUPPORTED WORK

Among certain subgroups in our society, employment problems are particularly prevalent and persistent. The sources of these problems are varied, but major factors include lack of experience and training, poor work habits, insufficient motivation, and discrimination by employers. Recognizing the serious consequences of these employment problems, both to the individuals themselves and to society, a consortium of Federal agencies, with the Department of Labor as the lead agency, undertook a major demonstration and evaluation of Supported Work programs. These programs provide work experience for a year or so, under conditions of gradually increasing performance standards, close and supportive supervision, and peer group support. The four target groups that were the focus of this demonstration include long-term recipients of AFDC, ex-addicts, ex-offenders, and young school dropouts.

The evaluation component of the demonstration was designed to measure the economic and social impacts of Supported Work. To facilitate this research objective, in 10 of the demonstration sites, eligible applicants to Supported Work were randomly assigned to either an "experimental" group and offered a Supported Work job or to a control group. Sample members were then scheduled to be interviewed at enrollment and at subsequent 9-month intervals for up to three years.

The main concern of this report is to assess the extent to which Supported Work is an effective program for mitigating the chronic employment problems faced by youth, particularly school dropouts and those from ethnic minority groups who face the most severe problems.[1]

In recent years, federally funded employment-related programs have served over 7 million youth per year, yet high unemployment rates have persisted. While the immediate and longer-run consequences of this high unemployment rate among youth, particularly minority youth and school dropouts, are uncertain, it has been suggested that in the short run, unemployment may increase the likelihood of involvement in crime (Singell, 1967; Mahoney, 1978; Funke, 1978; Elliot and Knowles, 1978) and the use of drugs (O'Donnell et al., 1976), and that in the longer-run it may tend to perpetuate employment problems (Osterman, 1978; Adams and Mangum, 1978; and DiPrete, 1978). This chapter discusses the sources of youth unemployment, public-policy approaches to alleviating the problem, evidence of the effectiveness of these various employment-related programs serving youth, and the role of Supported Work in the nation's youth employment policy.

A. SOURCES OF YOUTH UNEMPLOYMENT

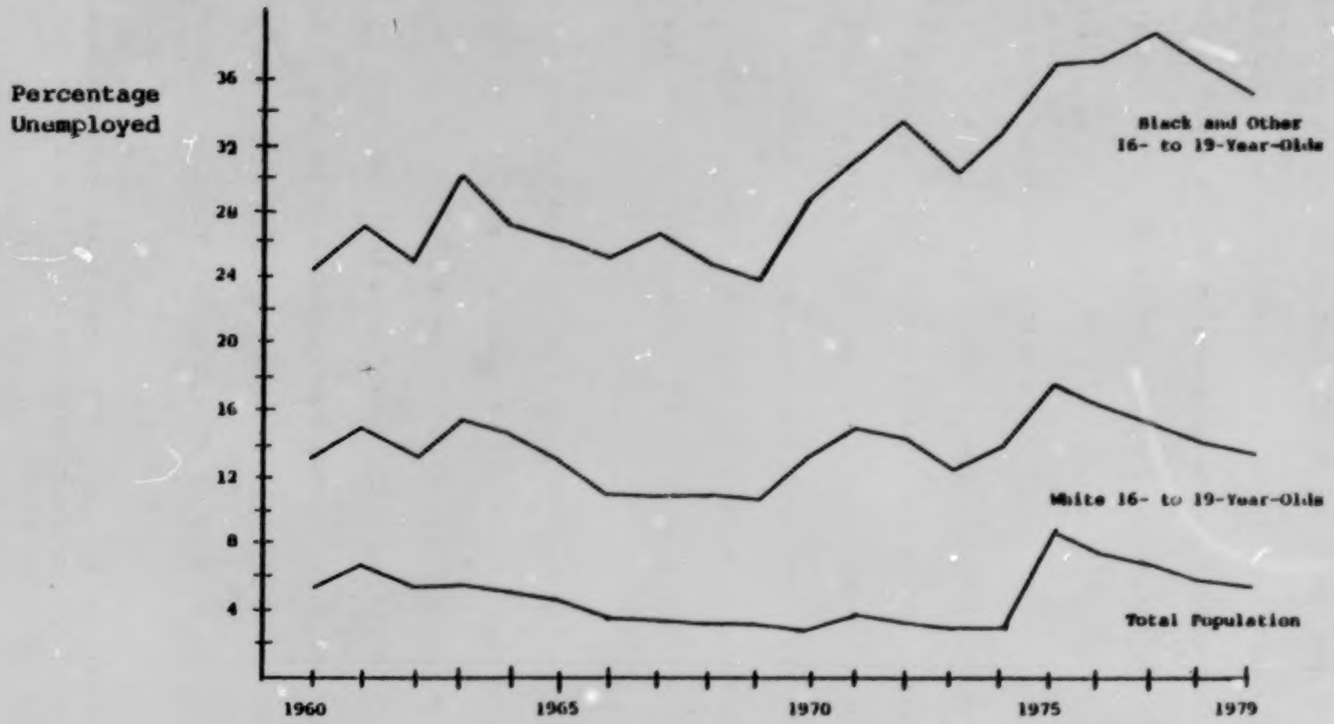
As can be seen in Figure I.1, the unemployment rate among youth rose substantially between 1960 and 1977, since which time there has been some decline coincident with an overall improvement in the employment situation. A number of factors are commonly cited as contributing

---

[1] The unemployment rate among black youth has been more than twice as large as that for white youth, and that among school dropouts is between 55 percent (for black youth) and 78 percent (for white youth) higher than among school graduates (U.S. Department of Labor 1978b and Bureau of Labor Statistics, 1978).

FIGURE I.1

TREND IN NATIONAL UNEMPLOYMENT RATES



SOURCE: *Employment and Training Report of the President*, Washington, D.C.: U.S. Department of Labor, 1978, and *NEWS*, Washington, D.C.: U.S. Department of Labor (various issues).

to the persistence of these high rates. One is the low skill levels and lack of experience of many of these youth, which, together with the rising minimum wages, may result in an unwillingness among employers to hire youth when adults are available for work (Barton and Fraser, 1978). Another cause may simply be discrimination on the part of employers (Diamond and Bedrosian, 1970). Yet another factor influencing this growth in unemployment is the increase in both the youth population and its civilian labor-force participation rate at a time when the participation rate among women, in particular, was also increasing. For example, between 1967 and 1977 the total labor force increased by 26 percent (from 77 to 97 million persons); two-thirds of this total increase was youths age 16 to 19 and adult females, who together had constituted only 40 percent of the labor force in 1967. These 20 million new entrants into the labor force were faced with an increase of only 16 million jobs and, for a variety of reasons noted previously, youth have been relatively less successful than other groups in competing for those jobs.

Exacerbating this general rise in youth unemployment has been an increase in the duration of unemployment spells. In 1966, only 11 percent of the unemployed 16- to 19-year-olds were unemployed for as long as 15 weeks; by 1977, this figure had risen to 15 percent. Since much of the youth unemployment appears to be due to movements in and out of the labor force, overall unemployment rates may be affected considerably with only small shifts in the duration of unemployment spells.1/

1/ Clark and Summers (1978) provide evidence of a substantial portion of youth unemployment being related to the high rate of movement in and out of the labor force.

In the past couple of years, there has been some general improvement in the economy which, together with the increased federal spending for youth employment programs, has led to some improvement in the youth employment situation: unemployment dropped from a high of 20 percent in 1975 to 16 percent during the first half of 1979. However, a disproportionate share of the gain in employment was among white youth who had completed high school.[1/]

B. POLICY MEANS TO REDUCE YOUTH UNEMPLOYMENT

In response to the persistently high unemployment rates among youth, there has been a continual expansion of programs designed to improve their employment opportunities. Such programs have tended to focus on increasing educational attainment, improving job skills, and providing work experience.

The first such programs were instituted under the Manpower Development and Training Act (MDTA) amendments of 1963, which emphasized job training and retraining programs. These were followed closely by the Neighborhood Youth Corps (NYC) and Job Corps, which were established under the 1964 Economic Opportunity Act. The NYC provided work experience to both in-school and out-of-school youth from low-income families, while Job Corps was a more comprehensive residential program which provided remedial education, skills training, work experience, counselling, and health care.[2/] These three programs together served an

---

[1/] See Tables A-5 and B-8 in The Employment and Training Report of the President, U.S. Department of Labor, Washington, D.C., 1978, and recent issues of NEWS: The Employment Situation, U.S. Department of Labor, Washington, D.C.

[2/] See Levitan and Johnston (1975) for a description of the Job Corps program.

average of 500,000 youth per year during 1963 to 1973, yet the total number of unemployed youth continued to rise, and the percentage unemployed remained fairly stable.

In recognition of the fact that the causes of and the means to reduce unemployment varied widely among segments of the unemployed population and among labor-market areas, the Comprehensive Employment and Training Act (CETA) was enacted in 1973 to permit much greater local autonomy for employment policy decision-making. This act provided funding for a number of programs to serve unemployed youth: the Employability Development Program (CETA, Title 1); Public Service Employment (CETA, Titles II and VI); Selective Segments Program (CETA, Title III), as well as the Job Corps (CETA, Title IV).[1/] By 1977, these CETA-sponsored programs, together with other federally funded programs, such as WIN (the Work Incentive program) and the HEW-sponsored vocational rehabilitation programs, served 2.7 million youth.

More recently, the Congress has greatly expanded employment services for youth through the Youth Employment and Demonstration Project Act (YEDPA) of 1977. A major directive of YEDPA, which provided jobs to nearly 400,000 youth in fiscal year 1978, is to improve our knowledge of the causes and potential cures of the employment problems of both in-school and out-of-school youth. The knowledge development plan for YEDPA includes both assessments of ongoing CETA programs, such as Job Corps and the Summer Youth Employment Program (SYEP), and demonstrations in the areas of public- and private-sector job development,

---

[1/] These CETA titles refer to those prior to the October 1978 reauthorization.

wage subsidies and wage vouchers, and programs aimed at various special segments of the unemployed youth--those still enrolled in school, delinquents, runaways, and school dropouts.[1]

C. CONCLUSIONS FROM EVALUATIONS OF OTHER YOUTH EMPLOYMENT PROGRAMS

In large part, these various policy approaches to alleviating youth unemployment problems have been motivated by an emerging literature that provides theoretical and empirical support for hypotheses concerning the general causes and consequences of unemployment. A brief review of this literature provides useful background information for subsequent discussions of the goals and expected effects of Supported Work programs for youth.

The first major category of literature discusses employment as a function of factors either inherent in or being attributed to the individual. According to human-capital theory, education, training, and labor-market experience constitute the important determinants of employment opportunities and labor supply.[2] The wage rate one can earn and, hence, one's supply of labor increases with the level of human capital. And high unemployment among youth, particularly young school dropouts, is thus explained as due to inadequate human capital.

---

[1] See The Employment and Training Report of the President, U.S. Department of Labor, Washington, D.C., 1979, and A Knowledge Development Plan for Youth Initiatives Fiscal 1979, U.S. Department of Labor, Washington, D.C., December 1978, for descriptions of these research and development projects.

[2] Becker (1962) provides the foundation for much of this literature. Others making significant contributions include Ben Porath (1967), Blinder and Weiss (1976), Mincer (1974), and Sewell and Hauser (1974).

This argument, especially when applied to the low-income population, has been challenged, however. According to one alternative theory--the dual labor-market theory--high unemployment is due to discrimination in the labor market rather than a lack of basic skills (Doeringer and Piore, 1971; Gordon, 1972; Hammermesh, 1971), with youth and those of limited education thought to be among the groups discriminated against. Yet another view of the workings of the labor market is that education and training are used as screening mechanisms to determine to whom job offers will be made and at what wage rates (Thurow, 1972); once again, youth will have fewer credentials than older members of the labor force, and so can be expected to experience relatively high unemployment rates. Still other literature points to such institutional constraints as minimum-wage legislation as important in determining employment by restricting demand (see, for example, Gramlich, 1976; Ragan, 1977; King, 1974).

A second category of literature discusses theoretical arguments and empirical evidence in support of the view that unemployment is attributable to differences in labor-supply decisions themselves: individuals weigh the monetary gains from working against the costs, which include both the value they place on alternative uses of their time and work-related expenses. A number of factors may affect the net monetary gains from working, including various tax rates (particularly the income tax and the implicit welfare tax rates), expenses for such items as child care and transportation, and foregone home production activities--none of which is expected to be particularly burdensome for youth as compared with other subgroups in the population. However, some have argued that youth place a relatively high value on leisure time, in part because many are still dependent on their parents for support and so have limited need for money. According to this argument, the relatively high labor-force entry and exit rates for youth are because they tend to be less willing than other groups to work steadily at prevailing wage rates (Osterman, 1978; Levitan and Belous, 1977).

Programs aimed at reducing unemployment among special segments of the population in which youth are highly represented or among youth specifically have been as varied as the theories concerning its causes. They include Job Opportunities in the Business Sector (JOBS), Opportunities Industrialization Centers (OI[illegible] the Concentrated Employment Program (CEP), Job Corps, the Neighborhood Youth Corps (NYC), various programs sponsored under CETA legislation (including the new YEDPA programs), and numerous small demonstration (or experimental) programs aimed at special subgroups of youth--particularly delinquents. While there is a sizable literature describing many of these programs, most programs, unfortunately, have not been carefully evaluated.[1] Furthermore, the methodologies employed in the various analyses that have been conducted often differ sufficiently that cross-program comparisons have no meaning. Despite these shortcomings, a brief summary of the nature and impact of some of these alternative strategies may provide some insights as to the likely effects of Supported Work for youth.

The JOBS program was designed to overcome employer prejudices and discrimination in the hiring of disadvantaged persons, among whom unemployed youth are overrepresented. Based on the limited data collected and analyzed, it has been suggested that the program may indeed have reduced prejudice among employers (Greenleigh Associates, 1970), but had only limited if any impact on employment, except among black women (Farber, 1971).[1]

---

[1] Those programs funded under the Youth Employment Demonstration Projects Act (YEDPA) of 1977 are too new to have been subjected to more than a descriptive analysis, even though more thorough analyses are planned.

In contrast to JOBS, the OIC program was oriented toward the provision of job training for youth. Evaluations of the OIC, which have been based primarily on in-program data for program participants only, are inadequate to assess the program's impacts.[2]

The CEP was designed to help youth overcome employer discrimination, primarily by providing youth with some employment experience and subsequent job-search assistance. One of the most comprehensive evaluations of CEP (Kirschner Associates, 1969), which relies on a comparison of participants' behavior before and after participation in CEP, suggests that the program led to post-program employment and wage gains. Other evaluations have arrived at more negative conclusions, however, perhaps because of methodological differences, cross-program differences, or both.[3]

---

[1] Perry et al. (1975) describe the weaknesses of these and several other evaluations of this program, which focus on somewhat different performance criteria and which, in some cases, arrive at different though not necessarily mutually inconsistent conclusions.

[2] For example, see Barry (1973), Olympus Research Corporation (1971), Scott (1969), Lawrence (1970), Peterson (1968), and Anderson and Young (1968).

[3] For example, Ashenfelter (1976) discusses the potential problems in using "before and after comparisons" in evaluating the effects of manpower programs on earnings gains.

The early NYC programs emphasized work experience and were shown to have had little overall effect in subsequent employment of participating out-of-school youth (Borus et al., 1970). In 1970, the program was restructured to place more emphasis on skill development and less on work experience. However, the economic impact of the restructured programs has not been carefully assessed.

Job Corps has been the focus of numerous evaluations. Since its inception in 1964, this program has focused on providing training, education, and other forms of supportive assistance in residential centers to youth from low-income families and with a background characterized by "cultural deprivation"--youth who, in terms of background characteristics, are very similar to those youth enrolling in Supported Work. Evaluations during Job Corps' first few years of operation provided mixed evidence of the program's economic impacts.[1] However, more recent studies employing carefully selected comparison groups and more rigorous evaluation techniques have shown that, in contrast to other employment and training programs for youth, the Job Corps program operating in the mid- to late seventies does seem to have had favorable economic and noneconomic impacts on participants. Among the most noteworthy findings are those pertaining to economic impacts (Mallar et al., 1979). On average, seven months after leaving the program, former Corpsmembers as compared with similar nonparticipating youth were more likely to be employed full time (27 versus 24 percent), earned higher average income ($212 versus $194 per month), and were less dependent on welfare.

---

[1] For example, see Louis Harris and Associates (1967), Cain (1968), and Engleman (1971).

In addition, a substantially lower percentage of Corpsmembers than comparison youth were arrested both during the time individuals were in the Job Corps program and subsequently. Impacts on job-seeking skills, nutrition and health habits, and goal-setting behavior have been similarly favorable (Abt Associates, 1979).

At this point it is premature to comment on the few preliminary results there are of evaluations of the more recent federally sponsored programs to combat youth unemployment, such as the Youth Employment and Training Program, the Summer Youth Employment Program, the Young Adult Conservation Corps, the Youth Incentive Entitlement Pilot Projects, the Youth Community Conservation and Improvement Projects, and numerous smaller demonstrations funded under Title III of CETA.[1/]

Based on this evidence from previous research, we are certain of only one thing--that youth unemployment has numerous causes that require a variety of treatments. One of the biggest jobs of future evaluation research will be to identify which policy or program prescriptions successfully address the employment needs of which group of youth.

D. SUPPORTED WORK: A SPECIAL APPROACH TO REDUCING YOUTH UNEMPLOYMENT

Since 1975, eight of the national Supported Work demonstration programs have been providing a special temporary employment opportunity

---

[1/] A number of other employment-related demonstration programs for youth have focused primarily on outcomes not related to employment, and so are not particularly relevant to this discussion. For example, see Blew et al. (1977) and Clarke (1974) for a description of demonstration programs aimed mainly at delinquent youth. These studies generally offer pessimistic conclusions concerning program impacts on delinquency and criminal behavior. However, they have serious methodological limitations ranging from small samples to inappropriate comparison groups.

to young school dropouts with longstanding employment problems.[1] The eligibility criteria for this Supported Work target group are summarized in Table I.1. Through a set of support structures, this program aims to prepare these young people for transition to employment in the regular labor market after a maximum of 12 to 18 months of participation in a Supported Work job. A supportive work environment is provided through work assignments in crews of peers and through close supervision by individuals who both understand the work histories and personal backgrounds of their crew members and will enforce gradually increased standards of attendance and performance. Supported Work jobs require low skills and they pay relatively low wages. However, there is an opportunity for individuals to increase their program earnings through bonuses and promotions for good performance and regular attendance.

Participation in such a program may mitigate a number of the factors thought to be related to the unusually high unemployment among this segment of the youth population. First, it provides an opportunity for participants to develop good work habits. Second, it permits them to build up a work history which should improve employers' willingness to hire them. Third, in some instances, these youth will acquire specific work skills through their job experience. Fourth, Supported Work includes job-readiness training and job-placement components designed to facilitate the transition to regular employment. Other important

---

[1] These programs are located in Atlanta, Hartford, Jersey City, Massachusetts, New York City, Philadelphia, West Virginia (five counties in the northwest region of the state), and Fon-du-Lac and Winnebago counties, Wisconsin. More recently, Supported Work programs in Bergen County, Atlantic County, and Trenton, New Jersey, and in Madison, Milwaukee, and the Western Dairyland Region, Wisconsin have been serving youth.

34

TABLE I.1

ELIGIBILITY FOR ENROLLMENT IN THE SUPPORTED WORK YOUTH TARGET GROUP

Criteria	Restriction
Age	17 to 20 years old
Recent Employment	Currently unemployed: no more than a total of 40 hours worked during the last four weeks
Employment History	No more than three months in one regular job (a job of 20 or more hours per week) during last six months No previous participation in either Supported Work or in the evaluation control group
School Status	No high-school or high-school equivalency diploma No enrollment in school during last six months
History of Contact with Law Enforcement Agency	A record of delinquency (for at least 50 percent of the youth enrolled)

SOURCE: Summary of the Second Annual Report on the National Supported Work Demonstration, New York: Manpower Demonstration Research Corporation, 1978.

NOTE: At the program's discretion, youth may be termed ineligible if they live in a residence or were referred by a source that required them to be employed.

consequences of the Supported Work experience may be reduced welfare dependence, less involvement in crime, and lower rates of drug use than would otherwise be expected. Program effects in the last two areas could also contribute to improved future employment opportunities.

To date, the only youth-oriented program for which there is convincing evidence of favorable post-program impacts is Job Corps, which is distinguished from the variety of other programs noted in its residential character, its more comprehensive provision of education, training, and employment experience, and its client population (which tends to be more disadvantaged).[1] Insofar as Supported Work serves a similarly disadvantaged client population and provides some special supportive features in addition to work experience, there is some reason to expect that it may improve the employment situation of participating youth. However, in its emphasis on work experience, Supported Work is more similar to the variety of programs for which there is more limited evidence of favorable post-program impacts.

E. ORGANIZATION OF THE REPORT

This report estimates the economic and social consequences for youth participating in five Supported Work programs. In addition to considering overall impacts, it addresses the issue of whether the program might be more efficient if targeted toward another group or subgroup of youth or whether such a program should be implemented under only certain economic and labor-market conditions.[1]

---

[1] About 87 percent of Job Corps enrollees have not completed high school, 70 percent are from minority ethnic groups, 33 percent are from families receiving public assistance, and 30 percent of the males have been convicted of a criminal offense (U.S. Department of Labor, February 1979).

The next chapter provides some basic background information about the sample and programs being studied, and discusses issues related to the research hypotheses addressed and the analytic techniques employed in the evaluation. Chapter III discusses employment-related outcomes of Supported Work, which are the primary focus of the study. Chapters IV through VII present findings related, respectively, to income sources and in-kind transfers, drug use, and involvement in criminal activities.

---

[1] Three companion reports present the results of evaluation of Supported Work's effects for long-term recipients of AFDC, ex-addicts, and ex-offenders. Another report presents a full benefit-cost analysis.

CHAPTER II

THE EVALUATION DESIGN AND THE SUPPORTED WORK PROGRAMS UNDER STUDY

The primary task of the evaluation component of the Supported Work demonstration is to determine whether participation in Supported Work favorably affects employment and other behavior. In order to interpret properly the findings reported in subsequent chapters of this report, it is important to understand the basic features of the evaluation design (including the resulting data base and sample characteristics) and to be familiar with characteristics of the particular Supported Work programs under study. After addressing these issues, key research hypotheses are described, the potential impact of local-labor market conditions on these hypothesized outcomes is considered, and, finally, the analytic approaches used in the study are outlined.

A. THE EVALUATION DESIGN

Determining the impact of Supported Work involves knowing what the behavior of participants would have been had they not participated in Supported Work. In most previous evaluations of employment and training programs, this has been accomplished either by using a comparison group of nonparticipants who have characteristics similar to participants or by comparing the behavior of sample members before and after participating in the program. Both methods of assessing program impacts have serious shortcomings[1] that can be overcome only by using a randomly selected control group. While not without risk and limitations, an experimental design was adopted for the national Supported Work demonstration, making it possible for researchers to estimate with a known degree of statistical confidence the impact of the Supported Work program.

---

[1] For example, see Ashenfelter (1976), Goldstein (1974), and Kerachsky and Mallar (1977).

1. Sample Enrollment and Data Collection

Applicants for youth target-group slots in five of the demonstration sites--Atlanta, Hartford, Jersey City, New York, and Philadelphia--were randomly assigned to either an experimental or a control group.[1/] Members of the experimental group were offered a Supported Work job for up to 12 or 18 months, depending on the site; members of the control group were not. The first youth subjected to random assignment applied to the Jersey City program in April 1975. Random assignment then continued through July 1977, by which time 570 youth had been enrolled in the experimental group and 682 assigned to the control group.[2/] Table II.1 indicates the enrollment in the youth sample over time in each site.

[1/] In all sites except Hartford, 50 percent of the applicants were assigned to the experimental group and 50 percent to the control group. In order to increase the research sample beyond that which would be generated by a 50-50 assignment given funded program slots, 40 percent of the Hartford applicants were assigned to the experimental group and 60 percent were assigned to the control group. Jackson et al. (1979) describe the random-assignment process in detail.

[2/] In general, random assignment resulted in a sample of experimentals and controls with similar characteristics (see Table A.1). However, statistically significant differences in age and in criminal histories were observed, thus highlighting the importance in the impact analysis of statistically controlling for such differences.

TABLE II.1

ENROLLMENT, BY SITE AND CALENDAR QUARTER

YOUTH SAMPLE

Enrollment Period	Atlanta	Hartford	Jersey City	New York	Philadelphia	Total Number	Total Percent
1975							
Second Quarter	--	--	14	--	--	14	1.1
Third Quarter	--	--	23	--	34	57	4.6
Fourth Quarter	--	--	64	--	7	71	5.7
1976							
First Quarter	--	31	10	--	16	57	4.6
Second Quarter	--	87	26	--	9	122	9.8
Third Quarter	--	109	25	--	22	156	12.5
Fourth Quarter	22	114	52	43	5	236	19.0
1977							
First Quarter	46	146	29	88	7	316	25.4
Second Quarter[a]	38	129	--	48	--	215	17.3
Site Total	106	616	243	179	100	1,244	
Percent of Overall Total	8.5	49.5	19.5	14.4	8.0		100.0

NOTE: These figures are only for youth who completed an enrollment (baseline) interview. This includes all but 8 of those subjected to random assignment.

[a] These figures include 40 July enrollments.

All members of both the experimental and control groups were scheduled to be interviewed by Mathematica Policy Research (MPR) staff at the time of their application for Supported Work, to determine their demographic characteristics, their employment history, welfare dependence, drug use, and criminal justice experiences. They were then scheduled to be reinterviewed 9 and 18 months later to collect post-enrollment data on items such as employment, welfare dependence, drug use, and criminal activities. Because all interviewing was terminated in March 1979, only 57 percent of the sample (those enrolled prior to 1977) were scheduled to be interviewed again 27 months after their enrollment, and 16 percent (those enrolled prior to April 1976) were scheduled to be interviewed both 27 and 36 months after their enrollment.[1]

Because of the differential length of follow-up among sample members, analysis of impacts for the various post-program periods have been based on different subgroups of enrollees:[2] analysis of outcomes during the first 18 months following enrollment have been based on those who completed an enrollment, a 9-month and an 18-month interview;[3]

---

[1] Most interviews were face-to-face and conducted either in MPR site offices (62 percent) or in the field (24 percent). However, a few were conducted in prisons and over the telephone, in which case questions about criminal activities and current drug use were not asked. For more detail on interviewing procedures and results, see Jackson et al. (1979).

[2] This sampling strategy was undertaken to maximize statistical power of the analysis, within a fixed budget (see Ruth et al., 1980, or MDRC, 1980, for further discussion of the sample design).

[3] Separate samples for the 1- to 9- and 10- to 18-month periods would have been slightly larger than that used. However, offsetting the advantages of larger samples were added complications of comparing results across time for somewhat different samples and higher computation costs.

analysis of impacts for the 19- to 27-month period is based on data for those who completed an enrollment interview plus a 27-month interview, regardless of whether or not they completed the assigned 9- and 18-month interviews; and the analysis of 28- to 36-month outcomes relies on data for those who completed an enrollment and a 36-month interview.[1] While interview response rates were higher than initially anticipated, between 20 and 30 percent of those scheduled to receive the various follow-up interviews failed to complete them, thus resulting in the analysis sample sizes reported in Table II.2.[2]

An important implication of the sample design is that the samples for analysis of various post-enrollment periods are distinguished from one another by the date an individual enrolled in the program: only the earliest enrollees received the longer-term follow-up interviews. Thus, to the extent that the individuals' characteristics, local labor-market conditions, and program characteristics varied across these enrollment periods, the estimates of longer-term results based on these particular subsamples may not be representative of those that

---

[1] Analysis samples for the 19- to 27- and 28- to 36-month outcome measures were defined in this manner in order to maximize the number of usable observations, as sample sizes for the later follow-up periods were marginal. Analysis of selected outcome measures (for example, some crime recidivism measures) do require that sample members will have completed all scheduled interviews. These deviations in the sample definition are noted where they occur.

[2] Table A.3 presents data on the number of various types of interviews that were assigned and completed. Brown (1979) has conducted an analysis of the impact of interview nonresponse on the evaluation results presented in this report, and concludes that the comparisons based on the completed interviews generally yield unbiased estimates of the true effects for the full sample of youth enrolled in the research sample (see Appendix B).

TABLE II.2

MAXIMUM SAMPLE SIZES FOR THE ANALYSIS, BY REFERENCE PERIOD OF THE OUTCOME MEASURE

YOUTH SAMPLE

	Months 1 - 18		Months 19 - 27		Months 28 - 36	
	Number	Percent[a]	Number	Percent[a]	Number	Percent[a]
Site						
Atlanta	83	9.6	15	2.9	0	0.0
Hartford	384	44.6	232	45.2	20	13.1
Jersey City	192	22.3	170	33.1	85	55.6
New York	135	15.7	18	3.5	0	0.0
Philadelphia	67	7.8	78	15.2	48	31.4
Latest Follow-up Interview[b]						
18 months	442	31.2	n.a.	n.a.	n.a.	n.a.
27 months	298	34.6	368	71.7	n.a.	n.a.
36 months	121	14.1	145	28.3	153	100.0
Total	861	100.0	513	100.0	153	100.0

NOTE: Actual sample sizes varied somewhat by outcome measure (see Appendix A.2), but generally included 90 to 99 percent of the cases in these totals. Most of the evaluation results are based on multivariate analysis that controls for preenrollment characteristics of experimental and control group members. Therefore, all analysis samples include only individuals who completed the enrollment (baseline) interview. Analysis of outcomes during the first 18 months after enrollment has been based only on individuals who, in addition to the enrollment interview, completed both the 9- and 18-month follow-up interviews (referred to hereafter as the 18-month analysis sample). Analysis of outcomes during months 19-27 and months 28-36 are based on individuals who completed the 27- and 36-month interviews, respectively, regardless of what other follow-up interviews they completed. They are referred to hereafter as the 27-month analysis sample and the 36-month analysis sample.

[a] These are percentages of the total youth sample in the appropriate reference period.

[b] These figures refer to the latest completed interview. A few sample members were scheduled to receive subsequent interviews but failed to complete them. Also, recall that some individuals in the samples for analysis of 19- to 27- and 28- to 36-month outcomes did not complete a previously scheduled follow-up interview. Thus, row totals vary.

n.a. means not applicable.

actually occurred for the full sample. Because of this fact, care has been taken subsequently to assess the extent to which the subsamples followed for varying periods of time differ in either their pre-enrollment characteristics or in their post-enrollment behavior.

Another concern with the demonstration data is the quality of information obtained through face-to-face interviews. As part of the evaluation effort, comparisons were made between interview and official records data on earnings, welfare receipt, and arrests. The general conclusion from these comparisons is that the quality of the interview data is quite good. Specific qualifications of the findings that are warranted, based on the results of these validation efforts, are noted and justified in the subsequent chapters.

2. Characteristics of the Supported Work Youth Sample

As seen from the data in Table II.3, the characteristics of the youth sample are similar to those specified in the eligibility criteria (see Table I.1 in Chapter I). About 60 percent were younger than 19, over one-third had completed fewer than 10 years of school, over one-fifth had never held a regular job, and of those who had held such a job, they had not had one for an average of nearly 11 months. In addition, 57 percent reported having been arrested and 38 percent reported a conviction.

These youth also exhibited other characteristics which identify them as being among those who are particularly likely to have limited employment opportunities. Probably the most noteworthy of these pertains to race; over 90 percent of the sample were from minority ethnic groups, among whom the national unemployment rate is about double the overall

TABLE II.3

PERCENTAGE DISTRIBUTIONS BY INDIVIDUAL CHARACTERISTICS AT ENROLLMENT

YOUTH SAMPLE

		Analysis Sample Used for Outcome Measures Covering		
Characteristic	All Enrollees	1- to 18-Month Post-enrollment Period	19- to 27-Month Post-enrollment Period	28- to 36-Month Post-enrollment Period
Years of Age				
Less than 18	28.6	27.9	26.9	27.5
18	31.0	31.7	31.7	29.6
19	24.3	24.7	25.7	30.3
Over 19	16.1	15.6	15.6	12.7
(Average Age in Years)	(18.3)	(18.3)	(18.3)	(18.3)
Sex				
Male	87.7	86.4	89.3	94.1
Female	12.3	13.6	10.7	5.9
Race/Ethnicity				
Hispanic	18.9	15.6	14.6	12.1
White, non-Hispanic	8.5	5.9	9.4	7.1
Black, non-Hispanic and other[a]	72.6	78.5	76.0	80.9
Years of School Completed				
Less than 9	15.4	15.1	16.4	18.3
9	23.7	24.3	22.6	22.9
10	35.2	35.4	34.7	30.1
11 or more	25.6	25.2	26.3	28.8
(Average years of school completed)	(9.6)	(9.7)	(9.6)	(9.7)
Years Since Last Enrolled in School				
Less than 1	34.7	36.2	34.0	40.7
1 - 2	26.4	26.8	30.2	24.3
More than 2	39.0	37.0	35.8	35.0
Reason Left School				
Expelled	15.2	15.2	15.8	11.1
Dropped Out				
Went to jail or in trouble with police	13.5	14.3	17.7	22.2
Wanted a job	28.2	29.0	29.2	35.3
Didn't like school	11.1	10.7	7.4	3.3
Other	31.9	30.8	29.8	28.1
Underwent Job Training in Past Year	13.4	14.1	13.6	17.7
(Average number of weeks)	(2.4)	(2.3)	(2.3)	(3.2)

TABLE II.3 (continued)

Characteristic	All Enrollees	Analysis Sample 1- to 18-Month Post-enrollment Period	19- to 27-Month Post-enrollment Period	28- to 36-Month Post-enrollment Period
Months Since Last Regular Job				
Less than 3	20.6	21.2	24.5	28.8
4 - 6	13.4	12.9	10.5	13.6
7 - 12	17.9	18.1	18.1	14.4
13 - 24	19.0	20.6	21.2	19.7
More than 24	5.7	5.4	3.3	3.0
No regular job	23.4	21.9	22.4	20.5
(Average months since last regular job, for those who had a job)	(10.9)	(10.8)	(10.2)	(9.3)
Current Family Status				
Married	4.0	3.7	5.5	5.9
Living with parents	64.9	69.6	71.8	76.0
Supporting dependents	10.4	10.4	8.7	5.8
Welfare Status During Previous Month				
Received welfare [b/]	13.4	12.5	11.9	11.3
Received AFDC	4.5	5.1	3.2	3.4
Received general assistance	5.5	4.7	5.6	7.5
(Average $ amount of welfare received)	(20.33)	(20.60)	(18.82)	(16.26)
Received Food Stamps	23.2	25.3	20.4	15.9
(Average bonus value of food stamps received)	(18.28)	(20.23)	(15.41)	(8.64)
Lived in public housing	26.1	28.4	26.5	15.7
Has Medicaid card [c/]	14.4	14.2	12.0	9.9
Housing Status				
Renting	75.6	75.0	70.5	60.9
(Average $ monthly rent)	(132.89)	(131.81)	(131.62)	(125.71)
(Average number of rooms per person)	(1.3)	(1.2)	(1.2)	(n.a.)
Drug Use and Drug Treatment				
Ever used marijuana	60.6	60.2	66.0	75.9
(Average number of months, if used [c/])	(38.5)	(37.2)	(35.1)	(32.4)
Ever used any drug other than marijuana	24.1	22.6	29.6	41.1
Ever used heroin	7.7	7.8	12.2	16.3
(Average number of months, if used [d/])	(31.0)	(30.6)	(35.2)	(45.1)
Used alcohol daily	6.0	5.8	7.1	8.3
Ever been in drug treatment	4.1	3.5	5.2	10.4

TABLE II.3 (continued)

		Analysis Sample		
Characteristic	All Enrollees	1- to 18-Month Post-enrollment Period	19- to 27-Month Post-enrollment Period	28- to 36-Month Post-enrollment Period
Criminal Offenses				
Ever arrested	57.3	54.2	63.6	64.1
(Average number of arrests)	(2.4)	(2.2)	(2.8)	(3.0)
Convicted	37.7	34.1	41.6	29.2
(Average number of convictions)	(0.7)	(0.6)	(0.8)	(0.5)
Time Since Last Incarceration				
Less than 1 year	19.3	17.1	19.0	20.4
1 to 2 years	4.5	4.9	6.4	5.6
More than 2 years	7.0	5.9	13.0	24.6
Never	69.3	72.1	61.5	49.3
Probation or Parole Status				
Currently on probation	25.0	24.0	27.6	26.1
Currently on parole	4.6	4.3	5.0	4.6
Number in Sample	1,244	861	513	153

NOTE: Unless otherwise indicated, all data refer to the full sample (i.e., zero values are included). Parentheses are used to indicate values that are not percentages. These data are from enrollment interviews conducted by MPR staff.

[a] Only two sample members are from "other" ethnic groups.

[b] Welfare includes AFDC, General Assistance, Supplemental Security Income, other welfare, and welfare income for which respondents were unable to identify the source.

[c] This information was obtained from the first follow-up interview but refers to the time of enrollment.

[d] This includes only those who used drugs a few times a month or more often.

average for youth. In addition, two-thirds of these young people had been out of school for more than a year when they enrolled in the program, and 29 percent of them reported having been expelled or having left school because of problems with the police. Few of these youth were married and supporting dependents; furthermore, nearly two-thirds were living with their parents. As noted previously, these latter characteristics are often associated with relatively low levels of attachment to the labor force.

These characteristics describe not only a group of youth who have serious labor-market disadvantages in relationship to the general population of young people, but also a more disadvantaged group than the typical youth enrolled in CETA programs, which are also targeted primarily at disadvantaged groups. For example, a much higher proportion of the Supported Work sample are members of black and other minority ethnic groups (91 versus 52 percent for CETA); virtually none of the Supported Work sample as compared with 67 percent of CETA enrollees had completed high school; furthermore, the Supported Work sample consists of school dropouts, while a sizable portion of the CETA participants are still enrolled in school; finally, only 2 percent of CETA youth are reported to have a criminal record as compared with 57 percent of the Supported Work sample.[1] The employment impact of the relatively more disadvantaged status of the Supported Work sample as compared with the typical CETA enrollee is evidenced by the fact that they were employed only 19 percent of the year prior to enrolling in the demonstration, while YEDPA youth (whose characteristics are similar to all youth CETA participants) were employed during 37 percent of the weeks that they were not enrolled in school (WESTAT, 1979).

---

[1] The CETA data referenced here are from WESTAT (1979), Tables 4-4 and 5-2. It should be noted that increasing emphasis has recently been placed on targeting CETA funds toward the most severely disadvantaged. As a result of this change in focus, the characteristics of CETA youth may become more like the Supported Work target group in the future (see U.S. Department of Labor, 1979).

The Supported Work sample, then, is certainly a group of youth who are not expected to compete successfully in the regular labor market without some special assistance. In the next section, we describe the nature of the experience and employment assistance provided to these youth through Supported Work programs.

B. THE SUPPORTED WORK PROGRAMS[1]

Supported Work is defined as a structured, transitional employment experience designed to help those with well-established employment difficulties gain the experience and develop the work habits necessary for successful participation in the workforce. The three features of Supported Work which, in combination, distinguish it from other employment and training programs are as follows:

- Peer group support
- Graduated stress
- Close supervision

While the demonstration planners and the program operators share the basic conviction that these elements are critical to the successful preparation of Supported Work participants for transition into unsubsidized employment, there is no consensus as to the most effective strategy for their implementation. Partly as a result of philosophical differences among program directors and partly due to local job development opportunities and constraints, there is considerable variation in the implementation of key programatic components. Table II.4 summarizes some of the key features of the five Supported Work programs from which the youth sample was drawn.

---

[1] For a more detailed description of the Supported Work programs, see MDRC (1978) or MDRC (1980).

1. General Characteristics

In designing the national Supported Work demonstration, a number of program characteristics were standardized and the variation in others was limited, in order to assure the implementation of the Supported Work model. The main similarity among the five programs in which the youth experimental group were offered employment is that they are all primarily work-experience programs, offering limited-term employment at relatively low wage rates to groups of disadvantaged workers.[1] All programs implement the concepts of close supervision, graduated stress, and peer-group support, and none provides significant amounts of ancillary services. The size of the programs, the target groups they serve, their job mix, and their implementation of the special Supported Work features vary considerably, however.

---

[1] On average, those youth who were employed during the year prior to their enrollment in Supported Work earned an hourly wage of $2.68, which is between 0 and 17 percent higher than the starting program wages. This is consistent with the demonstration's goals of setting program wages slightly below market opportunity wages for target-group members.

TABLE II.4

CHARACTERISTICS OF SUPPORTED WORK PROGRAMS ENROLLING YOUTH, BY SITE

	Site				
	Atlanta	Hartford	Jersey City	New York	Philadelphia
Total Number of Job Slots					
As of January 1976	46	53	123	0	49
As of June 1977	90	221	303	284	99
Other Target Groups Served	AFDC	AFDC Ex-Offenders	Ex-Addicts Ex-Offenders Ex-Alcoholics	AFDC	Ex-Addicts Ex-Offenders
Percentage of Job Slots Filled by Youth (as of June 1977)	38.9	32.5	20.8	28.4	11.1
Percentage Distribution of Youth's Project Days by Industry[a]					
Agriculture, forestry, fishing	0.0	12.1	0.0	0.1[b]	22.1
Construction	20.8	18.4	34.9	2.8	59.2
Manufacturing	1.8	15.9	8.0	0.0	0.0
Transportation, communication	0.0	1.6	15.1	0.0	0.0
Trade	0.0	4.0	7.0	2.3	1.0
Repair services	34.2	44.0	0.1	0.0	0.0
Other services (primarily business)	42.9	3.8	29.8	94.8	17.7
Crew Size					
Average crew size	3.1	7.4	6.7	n.a.	7.2
(Percentage in crew work)	(87.0)	(91.9)	(94.5)	(n.a.)	(90.3)
Bonus Policy					
Periodic performance bonus	$5 per week to $25 per month	$5 per week to $25 per month	$6 per week to $60 per 5 weeks	$5 per week to $25 per 6 weeks	$5 per week to $20 per month
Transition bonus	None	8 percent of gross wage	5 percent of gross wage	None	None
Supervision					
Percentage of youth supervised primarily by Supported Work	8.2	79.0	81.0	n.a.	87.7
Percentage of supervisors with prior experience training target groups served by Supported Work	75.0	10.3	13.6	n.a.	10.5
Termination					
Maximum Allowable Time in Supported Work	12 months	18 months	12 months	18 months	18 months
Percentage of all terminees placed in nonprogram jobs	16.3	28.4	25.7	16.2	18.4
Percentage of placements with jobs developed by Supported Work	68.7	62.7	48.6	9.1	26.9
Program Hourly Wage Rate					
Entry level as of January 1977	$2.30	$2.50	$2.68	$2.38	$2.48
As a percentage of the market opportunity wage[c]	67.4	63.3	60.4	87.4	66.7
Percentage of Youths' Paid Time Devoted to Ancillary Services	5.4	0.1	5.5	n.a.	0.9

SOURCE: These data are from MDRC (1978) and from various Supported Work MIS special reports. Unless otherwise noted they pertain to all target groups served by the site's program.

NOTE: Unless otherwise noted, data apply to the programs during their second contract year. This time covers the period when about 60 percent of the youth included in the analysis sample were working at their Supported Work jobs. Roughly 18 percent had completed their participation in Supported Work prior to the start of this contract period, and roughly 22 percent participated in the program for several months after the end of this period.

[a] Percentages will not always total 100 because industries for some project days are unknown. Agriculture refers primarily to landscaping.

[b] These data for New York are percentage distributions of all project days rather than just those worked by youths.

[c] Hollister et al. (1974) describes the estimation of market opportunity wages for the Supported Work Sample.

The Atlanta and Philadelphia programs, which enrolled 17 percent of the youth research sample, were considerably smaller and had substantially less expansion of program slots during the period under study than the other three sites. By June 1977, these two programs averaged only 90 to 100 total job slots, while the other programs each had between 200 and 300 slots.

None of these programs served youth exclusively. The Atlanta and New York programs filled a majority of their job slots (61 and 74 percent, respectively) with AFDC women. Jersey City devoted 80 percent of its slots to ex-offenders, ex-addicts, and ex-alcoholics, and Philadelphia filled 89 percent of its jobs with ex-addicts and ex-offenders. Only in the Hartford program were youth the predominant target group during this time period (47 percent of the Hartford jobs were held by AFDC women and ex-offenders).

A large share of the youths' employment in these five sites comprised jobs in the services industry--primarily repair (including auto repair in Atlanta and Hartford), building maintenance, and miscellaneous business services. Such jobs are particularly prevalent among the Atlanta and New York programs, which served only AFDC women in addition to youth. Both the Jersey City and Philadelphia programs employed a considerable portion of their youth in construction jobs, such as painting, building rehabilitation, and cleaning and sealing unoccupied housing. The Hartford program offered the greatest variety of jobs, with about half of their workers being employed in the service industry, but with sizable numbers working in parks maintenance, building construction and rehabilitation, and furniture manufacturing and repair.

2. The Implementation of Key Features of Supported Work

The manner in which these five programs have implemented the concepts of peer-group support was much more consistent across sites than their implementation of graduated stress and close supervision.

Peer-group support is designed to permit target-group members to improve their work habits and skills in an environment where they have the support of others who have similar disadvantages and anxieties about their jobs. This concept has been implemented in all sites through a commitment to crew work--groups of Supported Workers (not necessarily from a single target group) working together. However, the average crew size varied from 3.1 in Atlanta (where 13 percent of the workers, mainly AFDC women, were not assigned to crew work) to 7.4 in Hartford (where 22 percent of the crews included more than 10 persons).[1/] Furthermore, the stability of crews varied among sites, particularly in response to the length of work contracts: in sites such as Philadelphia, which tended to rely on short-term work contracts, crews were frequently dismantled and workers reassigned to other crews.

The concept of graduated stress is predicated on the belief that the target-group members served by Supported Work could not at the time of their enrollment meet the performance standards of the regular labor market; that they needed an opportunity to gradually increase their skills and improve their work habits. Thus, an important goal of the Supported Work programs was to develop jobs whereby initial

---

[1/] The consensus among program operators is that the optimal crew size is between 4 and 7 persons (MDRC, 1978), although evidence from a statistical process analysis does not support this belief (Hollister et al., 1979).

performance demands on the individuals were modest, but where they could be increased gradually over time until they resembled those the participant could expect to face in nonprogram jobs.

One of the main approaches to implementing graduated stress has been through ordering the job tasks sequentially--that is, by moving workers into successively more demanding work assignments. Another has involved the use of performance ratings such as those used in promotion and bonus-award decisions. The Jersey City program, in particular, makes use of the job-staging approach; the Hartford program places considerable emphasis on supervisor evaluations of a worker's performance in the determination of promotions. All programs have used inactivations, to some extent, as a means of permitting participants to attend to personal problems, such as those related to health, drug and alcohol abuse, and arrests and convictions.[1] Finally, both the Hartford and Jersey City programs have transition bonus policies, whereby individuals who are successful (either on their own or with the program's assistance) in finding a nonprogram job which they hold for a specified period of time (30 days in Hartford and 60 days in Jersey City) receive a bonus, the size of which varies with the length of time the individual has spent in Supported Work.

The third feature of Supported Work is close supervision. This supervision is designed primarily to transfer and develop technical skills and positive work habits and attitudes. To a lesser extent, supervisors are also expected to provide personal and job counselling to participants. In all but Atlanta, most workers are supervised by Supported Work program staff who have been hired largely because of their technical expertise and teaching skills.[1] Among the Atlanta work projects, a high percentage of the participants worked in jobs not under the direct supervision of the Supported Work program and, thus, were supervised primarily by the host agency, with a Supported Work program supervisor maintaining liaison with the nonprogram supervisor. While more compatible with the job-development outcomes in Atlanta, where many program jobs were single placements in public agencies, this method of supervision generally was not expected to be as effective as direct supervision by the Supported Work programs themselves, except under carefully selected situations.

---

[1] Programs may, under the demonstration's guidelines, permit participants up to three months of inactive time in addition to their maximum of 12 or 18 months of active participation in Supported Work.

C. HYPOTHESES CONCERNING PROGRAM IMPACTS

A number of primary hypotheses concerning participant outcomes underlie the basic Supported Work concept and the chosen target populations. In addition, the theoretical and evaluative literature cited previously suggests other hypotheses that pertain to potential program impacts and to differential impacts among subgroups of the youth target population. The primary hypotheses can be stated briefly as follows:

- Both during and after participation in Supported Work, experimentals will have more stable employment, work more hours, and earn more than their control-group counterparts.

---

[1] Some programs which did not enroll youth sought supervisors who, in addition to technical expertise, had experience working with the target-group members they were to supervise (MDRC, 1978).

- Experimentals will be less likely than controls to receive public-assistance benefits and, among those experimentals who continue to receive benefits, the average benefit level will be less than they would have received had they not participated in Supported Work.
- Experimentals will be less likely than controls to use drugs.
- Experimentals will be less likely than controls to engage in criminal behavior.

In addition, Supported Work could have a number of other important effects, such as influencing participation in education and training programs, household composition, and health-care utilization.

The rationale for the employment-related hypotheses has been discussed previously in the context of the design and goals of the Supported Work programs. The hypothesized reductions in dependence on public assistance are a corollary to the employment hypotheses.

Both sociological and economic theories of the causes of drug abuse suggest that an employment opportunity such as that offered by Supported Work may affect the prevalence of drug use. Sociological theories emphasize the importance of peer-group attitudes toward drug use versus other forms of behavior, such as working. Employment in Supported Work may be expected to alter the peer-group environment in such a way as to decrease the relative desirability of drug use vis-a-vis other activities, especially through the intensive and supportive supervision. Economic theories, in contrast, are concerned with the costs and desirability of drug use as compared with alternative forms of consumption and uses of time, and the hypothesized program impacts based on these theories are ambiguous. On the one hand, the Supported Work program tends to increase the opportunity cost of engaging in time-intensive activities such as drug use and, therefore, it would be expected to reduce the prevalence of use.[1] On the other hand, total income is expected to increase among participants in Supported Work, thereby permitting increased purchase of drugs by those who derive pleasure from them.

The hypotheses suggesting that successful integration of youth into the labor force might be expected to reduce their likelihood of participating in criminal activities are also based on both sociological and economic theories.[2] Among the sociological theories is one which claims that illegal behavior results from the disparity between the goals established and valued by society (primarily material or financial) and the means available to achieve these goals. Supported Work may increase the legitimate means for achieving these goals, thereby reducing delinquency and criminal behavior. A second model combines a labeling perspective with self-concept theories. An individual may have previously experienced contacts with agents of social control who have applied to him or her the label of "criminal" or "delinquent," with the result that the individual's self-image will align itself to this public image and lead to deviant or criminal behavior. However, once one becomes employed, his or her occupation often takes the role of "master status," submerging these other roles. A third perspective suggests that criminal behavior results from association with peers who define law violations as acceptable behavior. Thus, Supported Work and other forms of employment will tend to reduce criminal behavior by increasing contacts and interactions among others who are not oriented toward a delinquent or criminal life-style.

---

[1] Chien et al. (1964) indicate, however, that drug use may not be time-intensive in the sense that it reduces productive work time, except in cases of novice users or extreme addiction.

[2] See Piliavin and Gartner (1979) for a more detailed discussion of these theories of criminal behavior and their relationship to employment.

In contrast to these sociological theories, the economic theories of criminal behavior are based on a rational-choice model of human behavior. According to these theories, participation in illegal activities results from the individual's subjective evaluation of the costs (e.g., the probability of arrest, conviction, and incarceration) and the gains (e.g., financial benefits) of crime as compared with the costs and rewards of alternative uses of time (e.g., leisure and legitimate employment).[1] Improving one's employment opportunities would tend to reduce the desirability of criminal activity relative to employment by increasing the opportunity costs of engaging in crime.

In addition to the primary hypotheses noted above, Supported Work may be expected to affect participants in a number of other important ways. First, it may influence youth's decisions to participate in education and training programs. On the one hand, experimentals, as compared with controls, might decide to invest more heavily in education and training, either during or subsequent to their participation in Supported Work, to supplement their program work experience. On the other hand, by increasing employment opportunities for experimentals,

---

[1] For example, see Ehrlich (1973), Sjoquist (1973), Reynolds (1971), and Danzinger and Wheeler (1975).

Supported Work may lead youths to obtain less formal education and training than they otherwise would have.

Finally, Supported Work may affect the general quality of life of participants and former participants. This could occur either as a result of increased consumption of medical care (subsidized by employer insurance policies or not) or as a direct result of the more productive and stable life-styles of these individuals, as evidenced by changes in household composition and housing consumption, for example.

Any of the above program impacts, but particularly the primary impacts, may be expected to vary with the nature of an individual's Supported Work experience, with changes in local economic and labor-market conditions, and with the characteristics of the individual youth who participated in the program. Each of the five Supported Work programs in this sample may have different impacts on participants as a result of variations in the programs' characteristics, the labor market in which the programs operated, or the characteristics of the youth they served. The analysis estimates the differential impacts both across sites and among individuals of different ages, sex, race/ethnicity, levels of educational attainment, welfare dependence, household composition and living arrangements, prior work and job-training experiences, prior drug-use histories, and criminal histories. However, since most of the variations in local labor markets are across as opposed to within sites, only more casual inferences of the sensitivity of program impacts to local labor-market conditions can be supported by the data.

Results of the impacts of other employment and training programs for youth provide mixed evidence as to whether the above hypothesized effects of Supported Work will actually be realized. These other programs have had limited success in keeping youths in the programs--the average length of stay in both CETA programs and Job Corps has averaged about six months[1]--and their post-program impacts have been small, at best. It is possible that Supported Work can achieve more favorable results because of its special emphasis on peer-group support, close supervision, and graduated stress, and because of its targeting on those youth who may have the most severe employment problems. However, the importance of these special qualities of Supported Work may be particularly sensitive to the prevailing labor-market conditions: during periods of relatively high unemployment rates, the effects of factors such as employer preferences for adult workers and limited job experience may be most severe and, thus, the work experience and credentials provided by Supported Work may be particularly beneficial. Alternatively, it may be that a much stronger treatment than work experience will be necessary to substantially affect one's employability during such times. Because of our inability to directly estimate the importance of local labor-market conditions on the program's impacts, we view it as essential to provide the reader with a general description of the labor-market conditions and alternative employment opportunities available during the period of this study.

D. LOCAL LABOR MARKET CONDITIONS

Two factors related to the local labor-market conditions that prevailed during the period when individuals participated in Supported

---

[1] See MDRC (1978) and Mallar et al. (1979), respectively.

Work and subsequently when they were seeking nonprogram jobs may be expected to have influenced the effects of Supported Work. The first is that employment prospects, in general, varied from site to site and also showed quite different rates of improvement over time.[1] As can be seen from Figure II.1, the average unemployment rates during the demonstration period were highest in Jersey City (12.0 percent) and New York (9.3 percent) and lowest in Atlanta (5.8 percent) and Hartford (6.8 percent). While throughout 1975 and 1976 and in 1978, the area unemployment rates tended to fluctuate with little in the way of an overall trend, conditions did improve substantially in all sites during the first three quarters of 1977, particularly in Hartford and Jersey City, where the rates fell from about 9 to 5 percent and from about 15 to 11 percent, respectively.[2]

The second factor which may influence Supported Work's impact pertains to the existence of alternative programs and services. CETA-sponsored programs have provided the majority of the employment opportunities for those youths unable to find unsubsidized employment. These

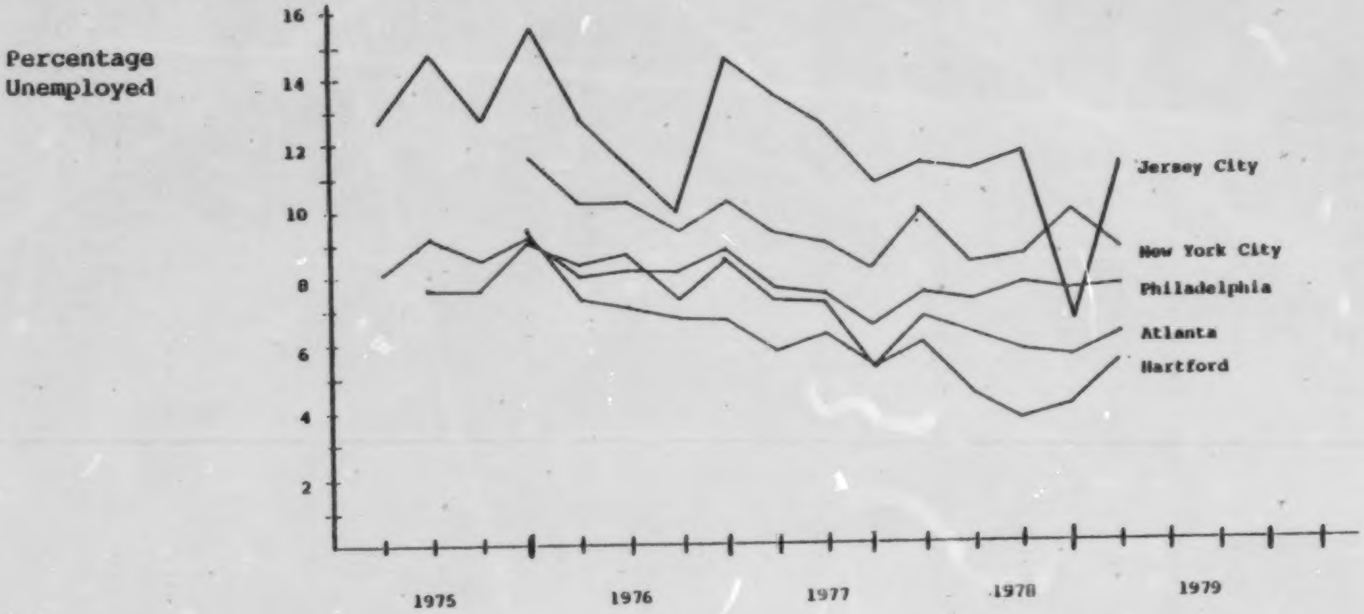
---

[1] Variation in market wage rate could also influence the impact of Supported Work. However, initial variation in program wages relative to area reference wages were not large, nor did they change much over time. Controls' wage rates during the first nine months following enrollment in the demonstration averaged between 10 (in Philadelphia) and 35 (in Atlanta and Hartford) percent higher than entry-level program wage rates, and there is no consistent pattern in these relative wage rates over time.

[2] As can be seen in Figure I.1, the youth unemployment rate, while substantially higher than the overall rate, tends to exhibit the same pattern of fluctuations over time. Thus, we expect the inter-site comparison based on overall rates to be indicative of the pattern that one would observe among youth.

FIGURE II.1

TREND IN AREA UNEMPLOYMENT RATES DURING THE PERIOD OF THE SUPPORTED WORK DEMONSTRATION



SOURCE: Various issues of Employment and Earnings, Washington, D.C.: U.S. Department of Labor and NEWS, Washington, D.C.: U.S. Department of Labor programs served about 1.6 million youth in fiscal year 1975, and by 1978 they were serving 2.4 million youth per year (see Table II.5). Most of this increase has resulted from expansion of existing youth programs and the creation of new ones under the Youth Employment Demonstration Projects Act (YEDPA). However, these new program jobs have not been distributed in proportion to either the youth population or the unemployed youth in the various sites. As can be seen from Table II.6, New York and Philadelphia have experienced the smallest increase in youth employment opportunities relative to their youth population, and Jersey City has experienced the greatest.1/ The expansions in Atlanta and Hartford are also moderately high, especially in light of their relatively low unemployment rates. As a consequence of these programs, which were aimed primarily at disadvantaged youth, employment opportunities for the Supported Work target population might be expected to be relatively more favorable in Jersey City, Atlanta, and Hartford than in the other sites.2/ As previously noted, these differential opportunities could potentially influence the impact of Supported Work.

Other programs that have differentially affected Supported Work participants in the various sites and thus warrant mention are the

---

1/ However, the unemployment rate in Jersey City was nearly twice the national average throughout most of the period under study.

2/ As will be seen in the subsequent chapter, the controls in Atlanta and Jersey City did tend to work more than average, particularly during the later time periods when these CETA-funded programs would have been in operation. However, for most of the follow-up period, there is no clear evidence that youths in these sites were especially likely to be employed in the CETA programs themselves.

TABLE II.5

NUMBER OF PARTICIPANTS IN CETA YOUNGER THAN 22

(Thousands)

	Title	Fiscal Year 1975	1976	1977	1978
I.	Employability Development[a]	695	982	732	650
II.	PSE-Structural[a]	54	56	72	44
III.	Summer Youth[a]	716	821	907	994
	YEDPA[b]	n.a.	n.a.	n.a.	377
IV.	Job Corps[c]	51	46	66	70
VI.	PSE-Countercyclical[a]	34	109	120	218
VIII.	YACC[d]	n.a.	n.a.	n.a.	27
TOTAL		1,550	2,014	1,897	2,380

[a] Source is Table F-7 of the Employment and Training Report of the President, U.S. Department of Labor, Washington, D.C., 1976 through 1979.

[b] These data are from Employment and Training Administration, OAM Transmittal Number 9-79, U.S. Department of Labor, Washington, D.C., April 25, 1979. They also include an estimated 30,000 participants in YIEPP.

[c] Fiscal year 1975 and 1976 estimates are based on data on number of Job Corps slots and average length of stay in Job Corps presented in Job Corps in Brief, U.S. Department of Labor, Washington, D.C., 1978. That for 1978 is based on the facts that there were 35,000 slots in the third quarter of 1977 (Employment and Training Report of the President, U.S. Department of Labor, Washington, D.C., 1979), and that the average length of participation is about six months (Mallar et al., 1979). The figure for fiscal year 1977 is from Chapter 3, Table 5 of the Employment and Training Report of the President, U.S. Department of Labor, Washington, D.C., 1978.

[d] These data are from Employment and Training Administration, OAM Transmittal Number 9-79, U.S. Department of Labor, Washington, D.C., April 25, 1979.

n.a.= not applicable.

TABLE II.6

PROGRAM SLOTS IN YOUTH EMPLOYMENT AND TRAINING PROGRAMS (YETP) AND YOUTH COMMUNITY CONSERVATION AND IMPROVEMENT PROGRAMS (YCCIP)

(Fiscal Year 1978)

	Number of Program Slots[a]			Program Participants	
	January-March	April-June	July-September	Number	As Percentage of Area Youth Population[b]
Atlanta	317	519	374	1,423	2.1
Hartford	0	420	393	630	2.9
Jersey City	1,224	434	89	1,857	6.2
New York	0	1,848	0	1,903	0.2
Philadelphia	543	1,103	940	2,041	1.0
Total U.S.	116,536	172,047	93,794	289,211	1.1

SOURCE: Youth Office, RAS OPRS Report 3, Department of Labor, Washington, D.C., 1978.

NOTE: During Fiscal Year 1978, 93 percent of the enrollees in youth programs were enrolled in the Youth Employment and Training Programs (YETP) or the Young Adult Conservation and Community Improvement Program (YCCIP). These are the only programs for which site-specific data were available.

[a] Slots are estimated as participants "on board" at the end of the quarter.

[b] Youth population estimates are based on 1970 Census data for the various cities, adjusted to reflect statewide trends in the youth population between 1970 and 1977.

Unemployment Compensation (UC) programs--the State Unemployment Insurance and Special Unemployment Assistance (SUA) programs. During the initial design of the Supported Work demonstration, program employment was specifically excluded from unemployment compensation coverage so that experimentals would not face a strong work disincentive as a result of UC benefits upon termination from the program. However, two events altered this initial design. One was the inclusion in the national demonstration of the ongoing New York Supported Work program, which, under state law, participated in the state UC program. The other was enactment of the SUA program in 1974 to provide unemployment compensation benefits to individuals who were employed in jobs not covered by the state UC program, but who otherwise met the state program's eligibility criteria. The Supported Work programs varied in their response to this event. The Hartford and Philadelphia programs actively attempted to prevent their former participants from gaining eligibility for SUA benefits, while a sizable percentage of youth in Jersey City, and to a lesser extent in Atlanta, did receive benefits upon program termination.[1] Because the SUA program was temporary (all claims were terminated on July 1, 1978) and because a national Supported Work program would undoubtedly have a uniform policy vis-a-vis participation in state UC programs, the estimated impacts of Supported Work based on the current sample are not generalizable to future experience. The short-term impacts of the New York, Jersey City, and Atlanta programs are almost certainly underestimates of the effects that would have occurred in the absence of UC receipt. However, the longer-run impacts are less certain. On the one hand, the availability of UC may promote longer average periods of job search that lead to better, higher-paying jobs; on the other hand, the job skills and credentials gained through Supported Work may decay during this extended period of job search.

---

[1] Receipt among the youth sample was not as prevalent as among the AFDC and ex-addict samples, however.

Together, the variation across and within sites in unemployment rates, alternative youth employment programs, and UC receipt may influence the effectiveness of Supported Work. Although no formal statistical evaluation of these potential influences is possible, subsequent discussions of findings (particularly with respect to site effects) do consider their relationship to prevailing labor-market conditions.

E. ANALYTIC APPROACH[1]

Most of the formal evaluation of Supported Work impacts on participants has been conducted using multiple regression analysis. Since random assignment to the experimental and control groups was strictly adhered to,[2] comparison of experimental and control group means should provide unbiased estimates of program effects.[3]

---

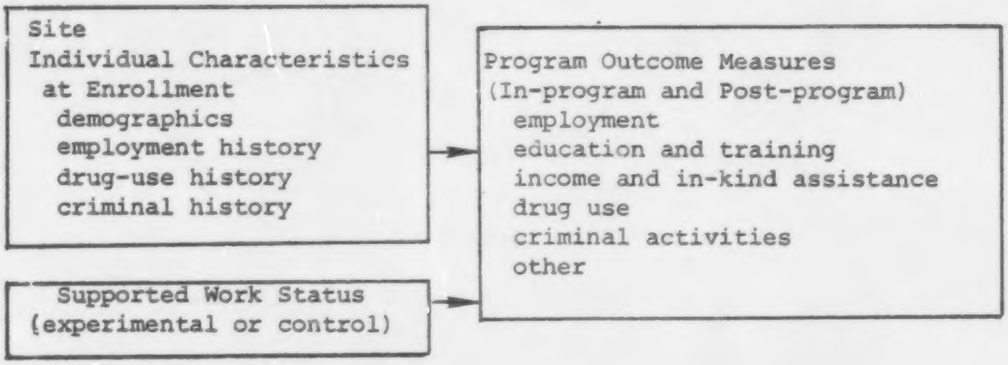
[1] Discussions of the various analytic techniques and statistical tests described here can be found in Hanushek and Jackson (1977) and other econometric text books.

[2] For evidence of the success of the random assignment procedure, see Jackson et al. (1978).

[3] By "unbiased" we mean that, on average, the estimate neither overstates nor understates the true effect.

However, regression analysis has two advantages. First, to the extent that measurable factors exogenous to the program treatment itself influence the outcome measures, regression analysis permits us to obtain estimates of program effects that have a higher degree of precision than those obtained through a simple comparison of means approach.[1] Second, regression analysis permits us to investigate easily whether program effects vary significantly among subgroups of the sample or among youth enrolled in different sites.

The most general model to estimate overall program effects can be depicted as follows:


Site
Individual Characteristics at Enrollment
demographics
employment history
drug-use history
criminal history
Supported Work Status (experimental or control)
Program Outcome Measures (In-program and Post-program)
employment
education and training
income and in-kind assistance
drug use
criminal activities
other

Formally, the impact of program participation is estimated through regression models of the form:

$$Y = a_0 + a_1X_1 + a_2X_2 + . . . + a_MX_M + bS + u$$

where Y is the observed outcome measure; X_m (m = 1, . . . , M) is a set of variables indicating the Supported Work site and the characteristics of the individual; S is a binary variable indicating whether the individual was assigned to the experimental group; and u is a random error term. The symbol a_m measures the impact of X_m on Y; and b is a measure of the overall impact of the program whose statistical significance level is measured by a t-test. (Appendix Table A.4a identifies the control variables used in the analysis and their means and standard deviations, and Table A.4b presents estimated coefficients on these control variables from selected regression equations used in the analysis.)

---

[1] The precision of the estimates is a measure of the likelihood that true program effects will not go undetected.

The extension of this basic model to estimate effects for subgroups of the sample is quite straightforward. The types of models estimated can be expressed formally as:

$$Y = a_0 + a_1X_1 + a_2X_2 + . . . + a_MX_M + b_0S + b_1SX_1$$

$$+ . . . + b_KSX_K + u$$

where X_k $(k = 1, . . ., K)$ is a subset of X_m. In this model, the program effect for a particular subgroup is measured by a linear combination of the b's; for example, if X is a set of binary variables to designate all but one of the Supported Work sites, then b_0 is the program effect for the omitted site and $b_0 + b_k$ is the program effect at site k. The statistical significance of the various subgroup effects can be measured by an F-test, as can tests of whether program effects vary among the subgroups (i.e., $b_1 = b_2 = . . . = b_k = 0$).[1]

---

[1] In subsequent tables, statistical significance of experimental-control differences both for total samples and for sample subgroups are denoted by asterisks. Statistically significant differences in the magnitude of program impacts among subgroups (that is, whether the hypotheses that the program impacts are similar for all subgroups can be rejected) are denoted by the pound symbol (#).

We should point out that these simple linear regression models may not yield estimates of program effects with desirable statistical properties in cases where the outcome measure is truncated (for example, hours worked) or in cases where it is dichotomous (for example, employed or not). Maximum likelihood techniques have been developed to account for these properties of the outcome measures, but are prohibitively costly for routine use in a project the magnitude of this one. Thus, since the standard regression techniques have repeatedly been shown to yield quite accurate estimates in most applications, we have tended to rely on this procedure and to selectively reestimate a number of the results using the maximum likelihood techniques probit (for dichotomous outcomes) and tobit (for bounded outcome measures) to test whether the basic conclusions are sensitive to this analytic constraint. It is also important to note that the results for some noncentral outcome measures are based on simple comparisons of means, since the cost of generating these estimates of program impacts is substantially lower that the cost of regression estimates, yet, because of the experimental design, they are still unbiased. We have noted throughout the report both the results of maximum likelihood reestimates of the program impacts and those places where simple comparisons of means have been used.

Regardless of the analytic technique employed (linear regression, maximum likelihood, or comparison of means), the discussion in subsequent chapters focuses on experimental/control-group differences in the various outcome measures. Since these differences are based on estimates of sample means, which are subject to sampling variability, we must consider the likelihood that the estimated difference between experimentals and controls is due to a true program effect as opposed to the random sampling variability. The statistical concepts which relate to this likelihood are the confidence interval around and the statistical significance of the estimated differentials.[1] In this report, we have adopted the standard procedures of indicating those estimated program effects which are significant at the 5 percent level on a two-tailed test--which means that there is less than a 2.5 percent chance that there was no program effect given the estimated differential. We also designate estimates of program effects that are significant at the 10 percent level, meaning that there is less than a 5 percent chance that the true effect is zero.

While we have adopted these standards for denoting "significant effects" in this report, there are two counterbalancing considerations which we also consider in interpreting the results. The first is the small probability that a difference as large as that which is significant would have been observed if the true effect were in fact zero. This means that one must expect the occurrence of occasional significant differentials, even in the absence of real program effects. The second is that failure to observe significant experimental-control differences does not necessarily mean they do not exist. It may simply mean there is so much sampling variability relative to the true effect that we cannot accurately estimate the size of the true effects.[1] In light of these considerations, in addition to adopting the standard criteria for denoting statistical significance, we have exercized some judgment in deciding which results or patterns of results are particularly worth noting in the discussion and interpretation of the findings.[2]

---

[1] The confidence interval, which is uniquely defined at various levels (the most common being the 95 percent level), is the range of values which has a 95 percent probability of containing the true value. That is, if repeated samples were drawn, and estimates and confidence intervals constructed for each, 95 percent of these intervals would contain the true value of the impact. If both ends of the confidence interval are greater or less than zero, an experimental-control differential is referred to as statistically significant (at the designated confidence level). For example, if we observe a differential whose 95 percent confidence interval is between $100 and $400 per month, there is only a .05 probability that the true differential is less than $100 or greater than $400.

---

[1] Increasing the sample size, of course, reduces sampling variability and, consequently, the likelihood that such true effects will go undetected. This concept of the likelihood that true effects will, in fact, be recognized as such in the analysis is commonly referred to as "statistical power."

[2] Yet another consideration in interpreting the results is that, in some cases, estimated program effects may meet the criteria of statistical significance but may be so small in magnitude that they are of little policy relevance or, in other cases, results which do not meet standard criteria of statistical significance may be so large that a policymaker may want to act on the basis of the findings.

CHAPTER III

EMPLOYMENT RELATED IMPACTS

In this chapter, we address a number of issues related to the success of Supported Work as a means of mitigating the employment problems of disadvantaged young school dropouts. The overall conclusion of the study is that Supported Work is not successful in improving employment prospects among this segment of the youth population, but it may be one way of reducing youth unemployment by providing employment for them in the short-run. Thus, the focus of this chapter is not only to present the evidence on which these conclusions are based, but also to provide some additional insight into the nature and the causes of and potential cures for the extraordinarily high unemployment rates among this segment of the youth population.

We begin by describing, briefly, the employment experiences of the control group members following their enrollment in the demonstration, in order to gain some general sense as to the nature of the employment problems faced by that part of the youth population to which Supported Work is directed. Subsequently, we describe the Supported Work experiences of the youth in the experimental group and their assessments of these experiences. In the sections detailing our results on the effects of Supported Work we discuss the following:

- The short- and long-run impacts of this program experience on overall employment rates, employment levels, earnings, and wage rates
- The extent to which program impacts vary across sites or among different subgroups of youth

- Whether the post-Supported Work employment characteristics of experimentals indicate any general improvement in their employment prospects relative to controls that may not have shown up in hours and earnings gains
- The impact of Supported Work on labor-market status and job-search behavior
- The effects of Supported Work on youth's acquisition of formal education and training

In the final section, we summarize the findings and present some suggestions for future program strategies that might further our knowledge of the problems, if not provide actual solutions.

A. EMPLOYMENT EXPERIENCES OF CONTROL GROUP MEMBERS

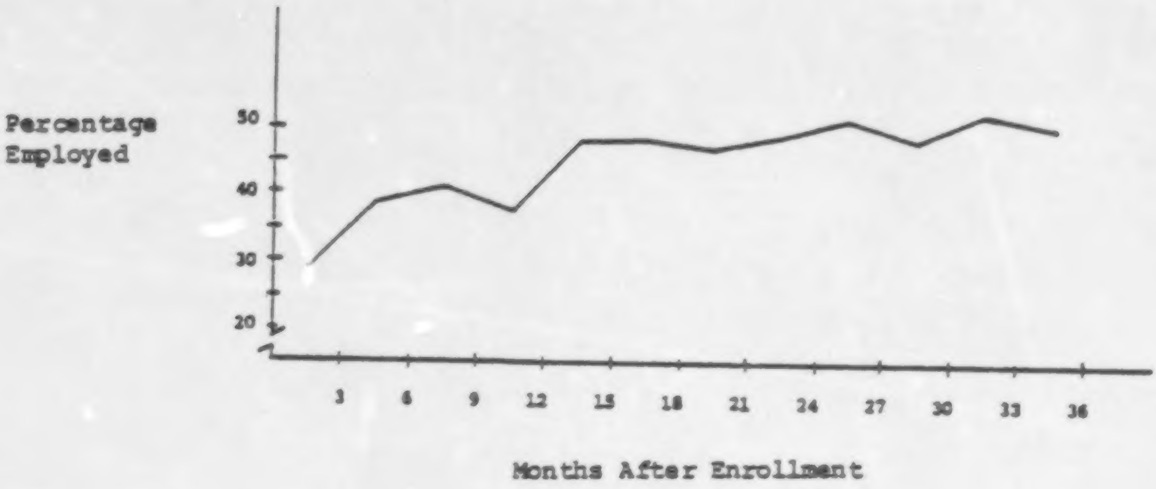
As noted in Chapter II, over 20 percent of the Supported Work youth sample had never held a regular job prior to enrolling in the demonstration, and only about one-fifth of them had held a job within the three months preceding their enrollment. However, as seen in Figure III.1, both the incidence of employment and the average number of hours worked per month tended to increase steadily throughout the follow-up period.[1/] During the first three months, 29 percent of the control youth were employed some of the time. For the total group, the average time worked was 31 hours per month (109 hours per month among those who worked). By the start of the third year after enrollment, however, half of the youth control group were employed. The average time worked was 70 hours per month (138 hours per

---

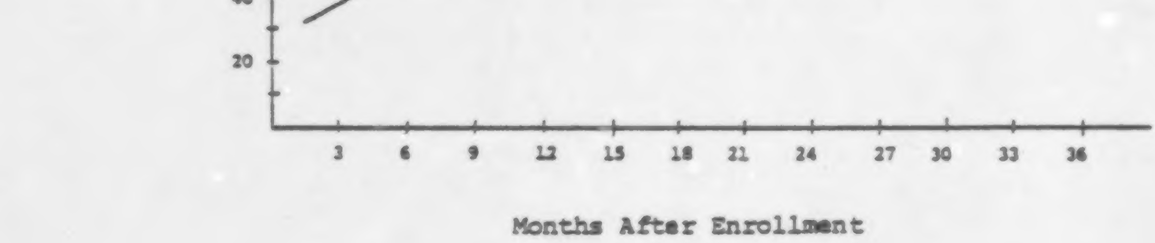
[1/] This upward trend in employment among the youth sample is far sharper than that for the other Supported Work target groups. By the end of the study period, youth controls were working 20 to 40 hours more per month than their counterparts in the other samples.

FIGURE III.1

A. TREND IN PERCENTAGE OF CONTROLS EMPLOYED (YOUTH SAMPLE)



B. TREND IN AVERAGE HOURS WORKED PER MONTH BY CONTROLS (YOUTH SAMPLE)



NOTE: These figures were plotted from data presented in Tables III.3 and III.4, respectively.

month among those employed).[1] This upward trend is due to three factors. One is the natural phenomenon by which some of those youth (all of whom were by definition unemployed at the start of the period) gained employment through a process termed "regression to the mean." A second factor is that these youth were getting older, and youth employment rates generally rise sharply with age.[2] The third factor is the general improvement in the labor market, particularly during 1977.

Few characteristics distinguish those with more post-enrollment employment experience from those with less. During the first 18 months after enrollment, controls in Jersey City and Atlanta tended to work significantly more than controls in the other sites, perhaps as a result of the relatively higher ratio of CETA jobs to the youth population in those sites.[3] Calendar time also tended to be an important determinant of controls' employment, with the later enrollees working significantly more than the earlier ones. In terms of personal characteristics, however, only sex and prior work experience seemed to be important. Males worked significantly more than females, and employment was positively and significantly related to prior work experience.[1] In part, this limited ability to distinguish among those who are more versus less likely to be employed is related to the fact that these youth tend to exhibit considerable instability in their employment. Those who were employed during the follow-up period held two jobs, on average, and the average spell of employment was just over five months.

---

[1] Eighty-three percent of the youth controls held some job during the post-enrollment period, broken down as follows: 78 percent of those with 18 months of follow-up data; 83 percent of those with 27 months; and 93 percent of those with 36 months of follow-up data.

[2] For example, in 1978 the national unemployment rate was 19 percent among 16- and 17-year-olds, 14 percent among 18- and 19-year-olds, and 10 percent among those age 20-24 (Table A-20, The Employment and Training Report of the President, U.S. Department of Labor, Washington, D.C., 1979).

[3] Between 7 and 10 percent of the Jersey City controls reported holding CETA jobs, and up to 30 percent reported holding public-sector jobs during each 9-month follow-up period. The comparable figures for Atlanta are lower but still substantially above the average for the other sites.

B. THE SUPPORTED WORK EXPERIENCE OF EXPERIMENTALS

The design of the Supported Work program is predicated on the belief that the principal factors contributing to the employment problems of the target populations are poor work habits and a lack of basic job skills. On this assumption, an opportunity to work in a supportive environment with gradually increased standards of performance would permit the development of good work habits while simultaneously building up basic employment credentials. From the outset it was recognized that there would be considerable variance in both the time it would take a target-group member to acquire work skills commensurate with general market standards and the amount of work experience that employers would view as convincing evidence of job readiness. Furthermore, there was a strong commitment to the concept that Supported Work is a transitional program. Thus, in an effort to balance the objectives of permitting sufficient program experience to acquire the intended program benefits and of ensuring that the programs not take on the character of sheltered workshops, the maximum allowable time one could participate in Supported Work was limited to 12 months in Atlanta and Jersey City and 18 months in Hartford, New York, and Philadelphia.[1]

---

[1] Surprisingly, the estimated increase in employment with age was relatively small and not statistically significant. The regression results from which these conclusions are drawn are presented in Appendix Table A.5.

On average, the youth stayed in Supported Work considerably shorter periods of time than allowed under program guidelines. As seen in Table III.1, 29 percent of the youth stayed three months or less,[2] and only 25 percent stayed as long as 12 months. The average length of participation was 6.7 months. However, there was considerable variation across sites, with those in Philadelphia staying less than four months, and those in Jersey City and New York staying an average of just over eight months. There was also considerable variation in the length of stay in the program among the different analysis samples. Most notably, those earliest enrollees for whom we have 36 months of follow-up data (the 36-month sample) stayed nearly one month longer than average. This longer average stay is due largely to the fact that over half of this sample is from Jersey City, where experimentals stayed in the program the longest; but it is also due, in part, to the more relaxed termination policies of programs during the early period of their operation when their job-placement procedures were still in the formative stages.

---

[1] This program time could be extended over a period of between 15 and 21 calendar months, respectively, if individuals had accumulated inactive time not due to punitive suspension. Furthermore, all individuals who enrolled prior to January 1, 1976 were permitted to participate in the program for up to 15 months, on the belief that programs had not yet developed strong job-placement services.

[2] Fewer than 3 percent of the experimentals failed ever to appear for their Supported Work jobs.

TABLE III.1

LENGTH OF PARTICIPATION IN SUPPORTED WORK

YOUTH EXPERIMENTAL SAMPLE

A. Percentage Distribution by Site

	Atlanta	Hartford	Jersey City	New York	Philadelphia	All Sites
Still in Program at End of Month						
3	74.5	70.5	84.9	64.4	43.8	71.0
6	57.4	45.8	62.4	49.3	18.8	49.4
9	48.6	31.5	46.3	36.5	11.1	36.1
12	23.4	16.3	36.2	37.0	9.4	24.8
15	2.1	1.8	11.7	31.5	9.4	10.0
18	0.0	0.0	0.0	24.1	8.3	4.6
(Average Number of Months in Program)[a]	(6.7)	(5.9)	(8.2)	(8.0)	(3.7)	(6.7)

B. Percentage Distribution by Amount of Follow-Up Data

	18 Months	27 Months	36 Months	Total Sample
Still in Program at End of Month				
3	70.5	71.2	72.6	71.0
6	46.7	50.4	56.5	49.4
9	32.0	39.2	41.5	36.1
12	26.7	18.6	32.3	24.8
15	11.4	5.0	16.1	10.0
18	8.1	0.9	1.8	4.6
(Average Number of Months in Program)[a]	(6.8)	(6.3)	(7.6)	(6.7)

NOTE: Samples are defined as specified in Table II.2, except that the 27- and 36-month samples must have completed all previously scheduled interviews.

[a] The average length of stay differs from the month of the first Supported Work termination for two reasons: some individuals do not begin their Supported Work job immediately upon enrolling in the demonstration sample, and some re-enroll after a period of inactivation.

While the overall average length of stay compares favorably with the average amount of time spent by youth in either Job Corps or CETA employment, and with the average length of time control youth stayed in their jobs (5 to 6 months), there is still some question as to why these youth stayed in the program such a short time in relationship to the program's policies of permitting 12 to 18 months of participation.[1/] Had these youth acquired the job skills necessary to find non-program employment in a significantly shorter period than anticipated would be required? Could they not meet the performance standards imposed even by the Supported Work program? Or did they simply not want to work? As can be seen from the data in Table III.2, each of these factors probably contributed to the overall result. Eighteen percent of the youth left prior to exhausting their allotted time in order to take another job or to enroll in an education or job-training program, 43 percent left for reasons related to their job performance (such as low productivity, failure to show up on time, conflicts with the boss or crew), and 31 percent reported more neutral reasons for having left (such as low pay and health, and child-care or transportation problems).[2/] Less than 3 percent of the sample reported having left the program because they did not want to work.

[1/] Length of stay does not vary with the program's policies on maximum allowable time in the program: among those in sites with a 12-month policy (Atlanta and Jersey City), youth stayed in the program an average of 7.7 months as compared with the overall average of 6.7 months.

[2/] These figures on types of terminations, which were generated from interview data, show a lower percentage of both positive and negative terminations and a higher percentage of neutral terminations than those reported in the Supported Work demonstration's Management Information System (MDRC, 1978 and MDRC, 1980). Explanations for these discrepancies include differences in the time periods and samples covered, as well as unavoidable differences in the actual definition of categories. Furthermore, the MIS data are based upon program operators' classifications of reasons as opposed to those of participants, and these two groups may have different interpretations of the reasons for a departure.

TABLE III.2

REASONS FOR LEAVING SUPPORTED WORK

YOUTH EXPERIMENTAL SAMPLE

A. Percentage Distribution by Site

	Atlanta	Hartford	Jersey City	New York	Philadelphia	Total Sample
Exhausted Allowable Time in Program[a]	22.9	0.0	25.0	8.9	0.0	8.5
To Take Another Job or Enroll in School or Job Training	14.3	19.7	16.7	12.5	38.5	18.0
Poor Performance[b]	28.6	45.8	31.3	51.8	53.8	42.9
Other[c]	34.3	34.5	27.1	26.8	7.7	30.6

B. Percentage Distribution by Amount of Follow-Up Data

	18 Months	27 Months	36 Months	Total Sample
Exhausted Allowable Time in Program[a]	8.4	5.5	27.8	8.5
To Take Another Job or Enroll in School or Job Training	15.7	19.1	33.3	18.0
Poor Performance[b]	42.8	45.5	27.8	42.9
Other[c]	33.1	30.0	11.1	30.6

NOTE: For definition of samples, see Table III.1.

[a] This includes individuals not leaving Supported Work to take another job, to enroll in school or job training, or because of poor performance, but who either spent the maximum number of months in the program or exceeded the maximum calendar time for participation.

[b] This category includes those terminated because of conflicts with the boss or crew members, use of drugs or alcohol, illegal activities or incarceration, absenteeism, poor punctuality, or low productivity.

[c] This includes reasons such as low pay and health, and child-care or transportation problems.

The likelihood of terminating for positive reasons was significantly related to having enrolled in the demonstration early, having more than 9 years of education, and not having a criminal history. The only factors significantly related to terminating for negative reasons were being in the New York site and having had limited or no prior work experience.[1]

Participants' assessments of Supported Work indicate that a majority of them (62 percent) felt that Supported Work did not prepare them to obtain a regular job.[2] This sentiment could partially account for the high rate of early terminations from the program. However, in general, efforts to identify factors related to length of stay in the program led to the conclusion that those who stayed in Supported Work for varying lengths of time left for quite different reasons. Those who enrolled during periods of relatively favorable labor-market conditions, and males, tended to have more employment opportunities and to stay in the program for shorter periods of time than otherwise similar youth enrolling during periods of worse labor-market conditions and than females, respectively. But controlling for other factors, we also observed that youth with the least education, job training, and prior work experience tended to be relatively short-term stayers despite their more limited alternative employment opportunities.[3]

---

[1] These findings are based on polytomous logit analysis of termination types (see Appendix Table A.7).

[2] Three-fourths of those who did feel Supported Work helped them said that it helped them by teaching job skills and trades. More detail on participants' attitudes toward the program is presented in Appendix Tables A.8a and A.8b.

[3] The regression estimates from which these conclusions are drawn are presented in Appendix Table A.6.

Still another question that arises is whether those 9 percent who exhausted their maximum allowable time in the program would have benefited from a slightly longer period of employment in Supported Work. There is no indication that there would be any long-term benefits from lengthening the program eligibility period. First, we observed no relationship between the length of time youth stayed in Supported Work and their subsequent employment gains.[1/] Furthermore, the post-program employment experiences of those who exhausted their allowable time in the program were more favorable than those of youth who left the program for other, "neutral" reasons and only slightly less favorable than those of individuals terminated for positive reasons.[2/]

Subsequent sections consider in detail the actual impact of this Supported Work experience on employment-related outcomes. In the context of these discussions, other aspects of the Supported Work experience, such as job-placement assistance, are discussed.

C. OVERALL EFFECTS ON EMPLOYMENT AND EARNINGS

Offering youth an opportunity to participate in Supported Work did have short-run benefits in terms of employment rates, employment levels,

---

[1/] Appendix C describes the formal analysis undertaken to ascertain whether or not program impacts were affected by the length of time an individual stayed in Supported Work.

[2/] For example, 40 percent of those who left Supported Work after exhausting their allowable program time found other employment within one month, compared with only 25 percent of those who left for other, neutral reasons. Over the period of observation, 80 percent of the "mandatory terminees," compared with 90 percent of the "positive terminees," entered regular employment, although the mandatory terminees spent an average of two months longer in job search than did the positive terminees (see Appendix Table A.9).

and earnings.[1] However, the results of this evaluation suggest that those impacts were due entirely to employment in Supported Work. Upon leaving their Supported Work jobs, experimental group members were about as equally likely as controls to find non-program jobs. The two groups averaged about the same number of hours of work per month and earned similar amounts in these jobs.

The general trend in outcomes can be seen from Figure III.2, which shows the average hours worked per month by experimentals as contrasted with controls. The large employment gains during the first few months declined sharply as experimentals left Supported Work; and by the start of the second year, when less than 20 percent of the experimentals were still in the program, there was essentially no difference in the overall employment levels of experimentals and controls.

This trend is the result of changes in both employment rates and hours worked by those who were employed. As seen in Table III.3, during the first three months after enrolling in the Supported Work demonstration, almost all of the experimental group (97 percent), as compared with only 29 percent of the control group, reported having some employment. Ninety-three percent of the experimental group held Supported Work jobs[2] and

---

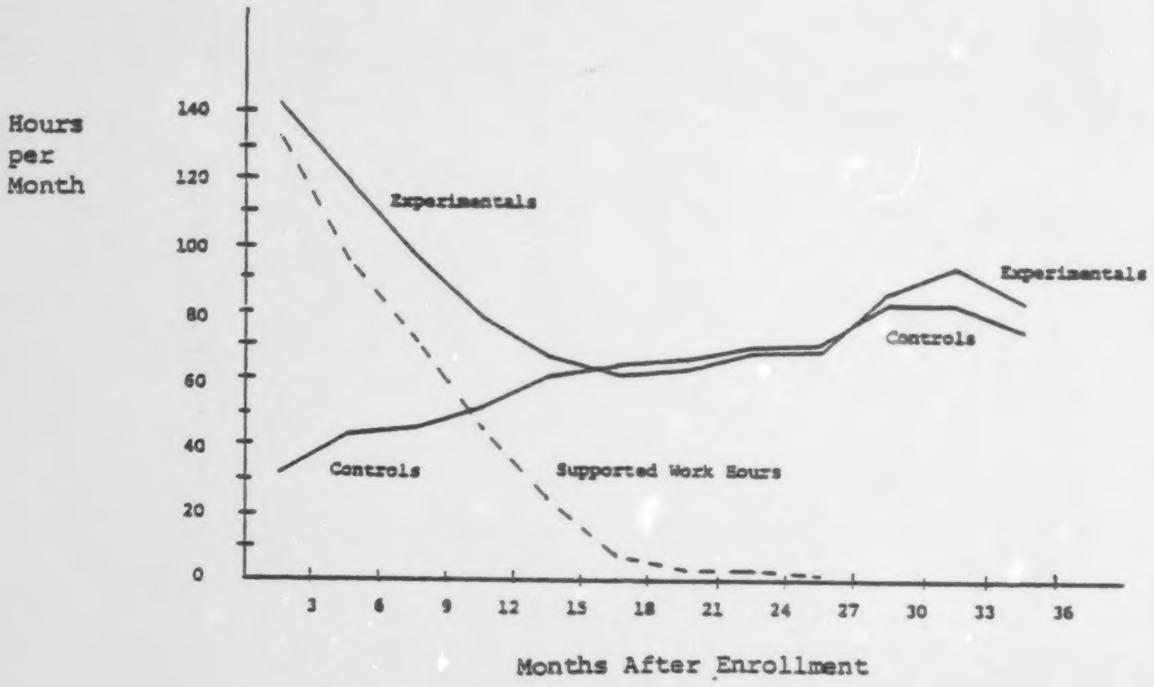
[1] Earnings data from Supported Work interviews were compared with those maintained by the Social Security Administration, primarily to assess the potential usefulness of Social Security data for a long-term follow-up of the sample (see Masters, 1979). The results of this comparison show 25 to 45 percent higher earnings reported in interviews, at least partly as a result of some employment not being covered by Social Security. However, estimates of experimental-control differences were similar for the two data sources.

[2] As already mentioned, less than 3 percent failed ever to show up for their Supported Work job. However, the follow-up interviews did not record data on periods of employment shorter than half a month, which is what accounts for the higher rate of those with no record of a Supported Work job.

FIGURE III.2

TREND IN HOURS WORKED PER MONTH

YOUTH SAMPLE



NOTES: Data plotted in this figure are reported in Table III.4.

Experimental-control differentials are significant only for months 1-12.

No experimentals should have been in Supported Work beyond the 21st month. That some report Supported Work hours during months 22-27 may be attributable to either data errors or to occasional failure on the part of program operators to terminate individuals promptly upon expiration of their eligibility period.

TABLE III.3

PERCENTAGE EMPLOYED

YOUTH SAMPLE

				Percentage of Experimentals with:[a]	
Months	Experimental Group Mean	Control Group Mean	Experimental-Control Differential	Any Supported Work Job	Only Supported Work Jobs
1 - 3	96.5	28.7	67.8**	93.0	84.7
4 - 6	81.8	38.9	42.9**	67.6	61.8
7 - 9	68.2	40.8	27.4**	49.9	44.3
10 - 12	54.8	36.0	18.8**	32.3	28.8
13 - 15	51.0	46.7	4.3	19.8	15.3
16 - 18	45.1	47.2	-2.1	8.2	5.5
19 - 21	45.2	42.4	2.8	1.7	1.7
22 - 24	47.0	49.1	-2.1	1.2	1.2
25 - 27	51.3	51.0	0.3	0.8	0.4
28 - 30	51.0	48.5	2.5	0.0	0.0
31 - 33	59.5	54.7	4.8	0.0	0.0
34 - 36	57.5	49.9	7.5	0.0	0.0

NOTE: Except where noted, all data are regression-adjusted. Control variables used in the regressions are listed in Appendix Table A.4. The samples used are defined in Table II.2.

[a] These data are not regression-adjusted. No experimentals should have been in Supported Work beyond month 21. That some reported program participation in later months reflects either data errors or failure by program operators to terminate individuals on schedule.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

50 percent of those with no program job were employed--reflecting, perhaps, some self-selection among those not choosing to enter Supported Work or to terminate early. During months 4 to 6, 82 percent of the experimentals were employed: nearly a third of them were no longer participating in Supported Work, but about 40 percent of these former participants held non-Supported Work jobs. During this same period, the percentage of controls who had some employment increased and the resulting differential in employment rates between the two groups was 43 percentage points.

This trend of a high rate of departure from Supported Work continued through the next two quarters, with successively lower percentages of experimentals finding non-program jobs upon leaving Supported Work and the percentages of controls who were employed staying reasonably constant (36 to 41 percent). Over months 13 to 27, however, overall employment rates were similar between experimentals and controls, ranging between 42 and 51 percent in the various time periods. This similarity in rates is due, in part, to a rise in the control group's employment of about 10 percentage points during the first quarter of this second year, but also to some continued decline in experimentals' employment through the 21st month.

Virtually all experimentals had left Supported Work by the 22nd month after enrollment, however, and there was, thereafter, a slight upturn in their employment rate: by the end of the third year after enrollment in the demonstration, 58 percent of the experimentals and 50 percent of the controls reported some employment.[1]

---

[1] It should be noted that this 8 percentage-point differential is based on few observations (153) and is not significantly different from zero.

As we have seen from Figure III.2, the pattern of results for total hours employed (both Supported Work and non-Supported Work jobs) parallels those for employment rates.[1] As indicated by the data in Table III.4, the main source of the decrease in the experimental-control differential over the year following enrollment is the sharp decline in Supported Work employment, from an average of 131 hours per month during the initial quarter to 46 hours per month during the fourth quarter. Only about one-fourth of this decrease in program hours was offset by the rise in experimentals' non-program employment. Furthermore, controls increased their employment by 61 percent over this same period, from 31 to 50 hours per month. Thus, 75 percent of the decline in the experimental-control differential in hours worked during this first year is due to a reduction in the employment of experimentals, and the remaining 25 percent is attributable to a rise in the employment of controls.

In the 12- to 15-month period after enrollment, nearly 60 percent of those who had stayed in the program for as long as 12 months left, largely because they had exhausted their allowable time in Supported Work; and the immediate post-program employment experiences of this group tended to be less favorable than the average for controls. Consequently, the experimental-control differential in hours worked dropped essentially to zero. It was not until the 28- to 36-month period that experimentals showed any evidence of employment gains relative to controls. Even then,

---

[1] Appendix Table A.10 presents a distribution of the average number of hours worked and earnings per month during each 9-month period following enrollment.

TABLE III.4

HOURS WORKED PER MONTH

YOUTH SAMPLE

				Supported Work Hours	
Months	Experimental Group Mean	Control Group Mean	Experimental-Control Differential	Number[a]	As Percentage of Total Hours of Experimentals
1 - 3	143.3	31.2	112.1**	131.1	91.5
4 - 6	120.1	43.9	76.2**	96.2	80.1
7 - 9	97.1	44.8	52.3**	70.5	72.6
10 - 12	79.4	50.2	29.2**	46.2	58.2
13 - 15	67.2	62.2	5.0	21.4	31.9
16 - 18	60.4	61.3	-0.9	8.8	14.6
19 - 21	64.4	63.6	0.8	2.4	3.8
22 - 24	69.6	70.0	-0.4	2.0	2.9
25 - 27	69.1	70.4	-1.3	0.6	0.9
28 - 30	87.2	83.0	4.2	0.0	0.0
31 - 33	92.8	82.2	10.6	0.0	0.0
34 - 36	83.3	75.8	7.5	0.0	0.0

NOTE: See note to Table III.3.
Overall experimental-control differentials and control-group means may vary somewhat from the averages reported in Tables III.6, III.7, and III.8 due to slight differences in samples.

[a] These data are not regression-adjusted. No experimentals should have been in Supported Work beyond month 21. That some reported program participation in later months reflects either data errors or failure by program operators to terminate individuals on schedule.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

however, the differentials observed were neither large nor significantly different from zero.[1]

The results for earnings (gross) are presented in Table III.5. For the first 18 months following enrollment, these results parallel those for employment rates and hours worked.[2] However, over the 19- to 36-month period, there was a shift in relative wage rates from experimentals earning somewhat more per hour than controls to their earning substantially less ($3.93 versus $4.37) in the last quarter of the third year.[3] The result of this shift is that, while point estimates of hours and employment-rate differentials are positive (though not significant) in the later periods, those for earnings are negative but, again, not significantly different from zero.

While the overall results indicate quite clearly that Supported Work has not significantly influenced post-program employment of youth, the pattern of results observed for the different outcome measures does raise a number of questions. The two main questions are whether the

---

[1] Tobit estimates of experimental-control differentials were quite similar to those reported in Table III.4; they indicate that in each time period, roughly half of the differential in hours worked is due to differences in employment rates and the other half to differences, among those employed, in hours worked (see Appendix Table A.11).

[2] If all earnings data are inflated or deflated to equivalent dollars as of the fourth quarter of 1976, experimental-control differences are slightly smaller. On average, the absolute value of the differential will be about 10 percent smaller during the first nine months, 5 percent smaller during the second, 18 percent smaller during the third, and 11 percent smaller over the fourth. Estimates are that 10 to 15 percent of gross earnings were paid in state and local income taxes and in Social Security taxes.

[3] These wage rates are calculated by dividing the average earnings per month (Table III.5) by the average number of hours worked per month (Table III.4). For the reader's convenience, these wage-rate calculations are presented in Appendix Table A.12.

TABLE III.5

DOLLAR EARNINGS PER MONTH (GROSS)

YOUTH SAMPLE

				Supported Work Earnings	
Months	Experimental Group Mean	Control Group Mean	Experimental-Control Differential	Dollars[a]	As Percentage of Total Earnings of Experimentals
1 - 3	389.52	100.15	289.37**	353.90	90.9
4 - 6	340.76	140.51	200.25**	266.80	78.3
7 - 9	284.40	138.24	146.16**	197.20	69.3
10 - 12	255.24	163.51	91.73**	134.10	52.5
13 - 15	218.79	211.16	7.63	63.33	28.9
16 - 18	208.78	213.04	-4.26	28.23	13.5
19 - 21	246.22	220.85	25.37	9.33	3.8
22 - 24	270.77	256.01	14.76	7.75	2.9
25 - 27	265.98	268.05	-2.07	1.63	0.6
28 - 30	300.95	323.53	-22.58	0.00	0.0
31 - 33	323.60	347.73	-24.13	0.00	0.0
34 - 36	287.13	331.59	-44.46	0.00	0.0

NOTE: See note to Table III.3.
Overall experimental-control differentials and control group means may vary somewhat from the averages reported in Table III.6 due to slight differences in the samples.

[a] These data are not regression-adjusted. No experimentals should have been in Supported Work beyond month 21. That some reported program participation in later months reflects either data errors or occasional failure by program operators to terminate individuals on schedule.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

results estimated for months 19 to 27 and 28 to 36 are generalizable to those that could be expected for the full sample, and whether the program's impact was significantly affected by the fact that some experimentals received unemployment compensation upon leaving Supported Work. We consider these below, as well as other issues that might explain the pattern of results.

1. Generalizability of Later Period Results to Full Youth Sample

As can be seen from Figure III.3, program impacts differ substantially among those groups with various amounts of follow-up data,[1] suggesting that the longer-term results estimated from the subsample who were assigned and completed a 36-month interview may not be generalizable to the entire demonstration youth sample.[2] Two factors are particularly relevant to this conclusion. One is the overrepresentation of Jersey City in the 36-month sample[3] and the other is the differential in employment patterns among controls in the various subgroups.

---

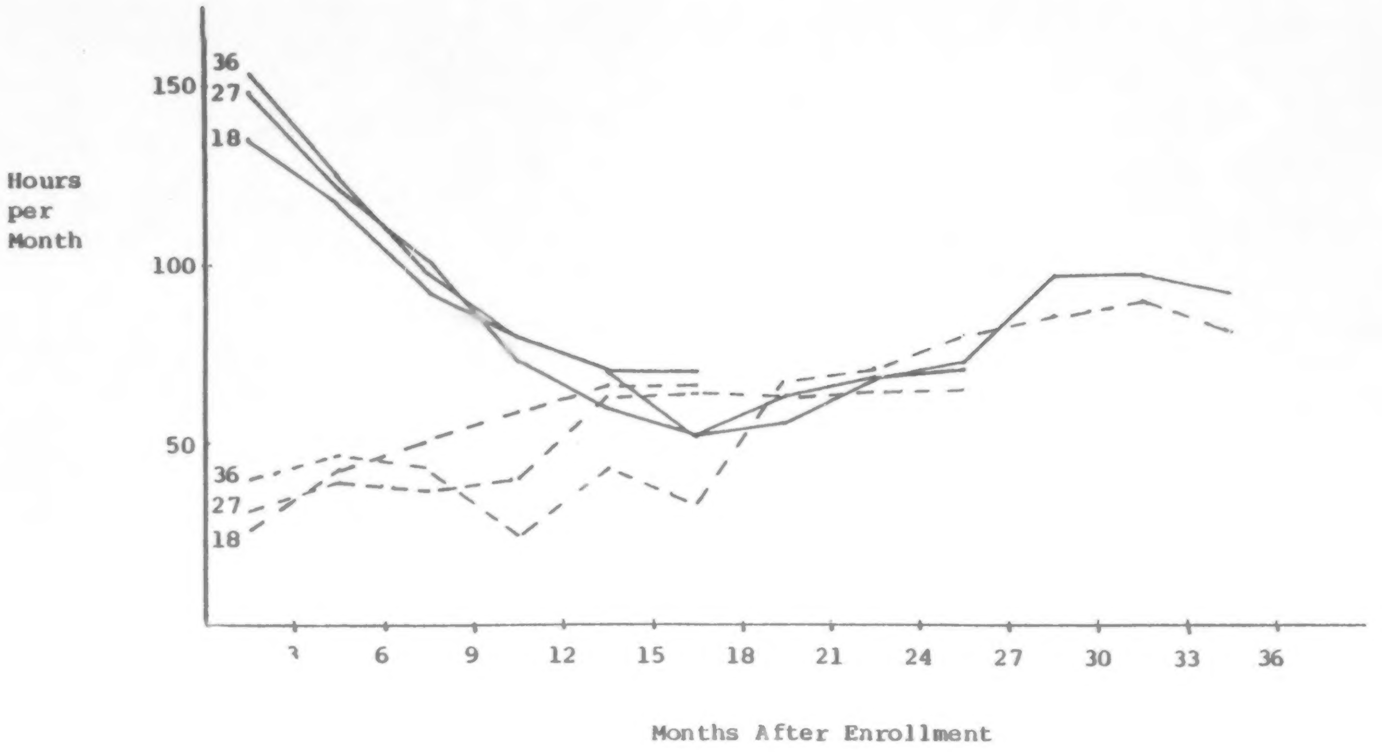
[1] For computational ease, subgroups were defined according to the amount of available data rather than the types of interviews scheduled to be completed. The slight differences in sample definitions have been found not to affect the conclusions. As noted in Chapter II, 35 percent of the youth sample was followed for 27 months and 14 percent was followed for 36 months. The remainder of the sample completed only an 18-month interview.

[2] To ensure that changes in results over program time are due only to changes in experimentals' and controls' behavior and not to changes in sample composition, results plotted in Figure III.3 pertain only to those individuals for whom continuous data throughout the follow-up period are available.

[3] Jersey City enrollees constitute 57 percent of the subsample with 36 months of data but less than 20 percent of the total youth sample.

FIGURE III.3

TREND IN HOURS WORKED PER MONTH BY SUBSAMPLES WITH VARYING AMOUNTS OF FOLLOW-UP DATA



NOTE: These data are not regression-adjusted. Figures used to plot the graph are presented in Appendix Table A.13.

KEY: ________ Experimentals
– – – – – Controls

18 = 18 Months of Follow-up Data
27 = 27 Months of Follow-up Data
36 = 36 Months of Follow-up Data

Over the 10- to 18-month period, differentials tended to be substantially larger for those with 36 months of follow-up data than for those whose last interview was 18 or 27 months after enrollment, largely as a result of differentials in the employment levels of controls. However, by the 19- to 27-month period, a more complicated pattern of results emerged.

For example, the positive 16-hour differential observed for the 36-month cohort in months 16 to 18 disappeared as controls (primarily among the Jersey City sample) increased by 89 percent their average hours of work, while experimentals increased their employment at a more modest rate. The sharp upturn in results between months 25 to 27 and 28 to 30 for this 36-month sample is attributable to an extraordinarily large increase in experimentals' employment relative to controls' (30 versus 4 hours per month), again a trend due primarily to changes in behavior of the Jersey City sample.[1] In contrast, experimental-control differentials for the subgroup with 27 months of follow-up data became increasingly positive over the 19- to 27-month period, as experimentals' employment increased faster than controls' and than that of experimentals in the 36-month subgroup. The lower employment among controls and higher employment among experimentals in the 27-month subgroup relative to those in the 36-month group is due entirely to a lower representation of the Jersey City sample in the 27-month cohort. Abstracting from this confounding influence of the Jersey City sample, the general pattern, found for other Supported

---

[1] Controlling for site, controls' employment in months 19 to 27 is lower among the 36-month cohort and experimental-control differentials are larger. The peculiarities of the Jersey City 36-month subsample are likely to be due in part to extraordinarily high UC receipt among experimentals in this group relative to controls and relative to experimentals in the 27-month cohort (see Figure III.4).

Work target groups, of more favorable experimental-control differentials being observed among those groups where controls work relatively little is observed for the youth sample, as can be seen in Figure III.4.

A more comprehensive view of the nature and extent of subgroup differences in outcomes can be seen from Table III.6. As noted previously, those controls with 36 months of follow-up data tended to work and earn substantially less during the 10- to 18-month period than did controls followed for shorter periods of time. However, these data indicate that controls in the 36-month subgroup worked at jobs paying higher than average hourly wage rates ($3.74 per hour versus $3.42 to $3.56 per hour for those with 18 and 27 months of data), while experimentals in the 36-month group worked at substantially lower wage rates than did experimentals with less follow-up data. Thus, earnings gains in months 10 to 18 are small relative to the hours gains for this subgroup.

Between months 10 to 18 and 19 to 27, among those for whom data for both periods are available, the observed sizable increase in the average employment among control group members (from 51 to 68 hours) and the small change in the average employment among experimentals (from 66 to 68 hours per month)[1] was accompanied by a shift in the types of employment of the two groups of workers over time. Experimental group members tended to shift from Supported Work to non-Supported Work jobs, and those in the 36-month sample working in non-program jobs earned substantially higher average hourly wage rates in the 19- to 27-month period than did those

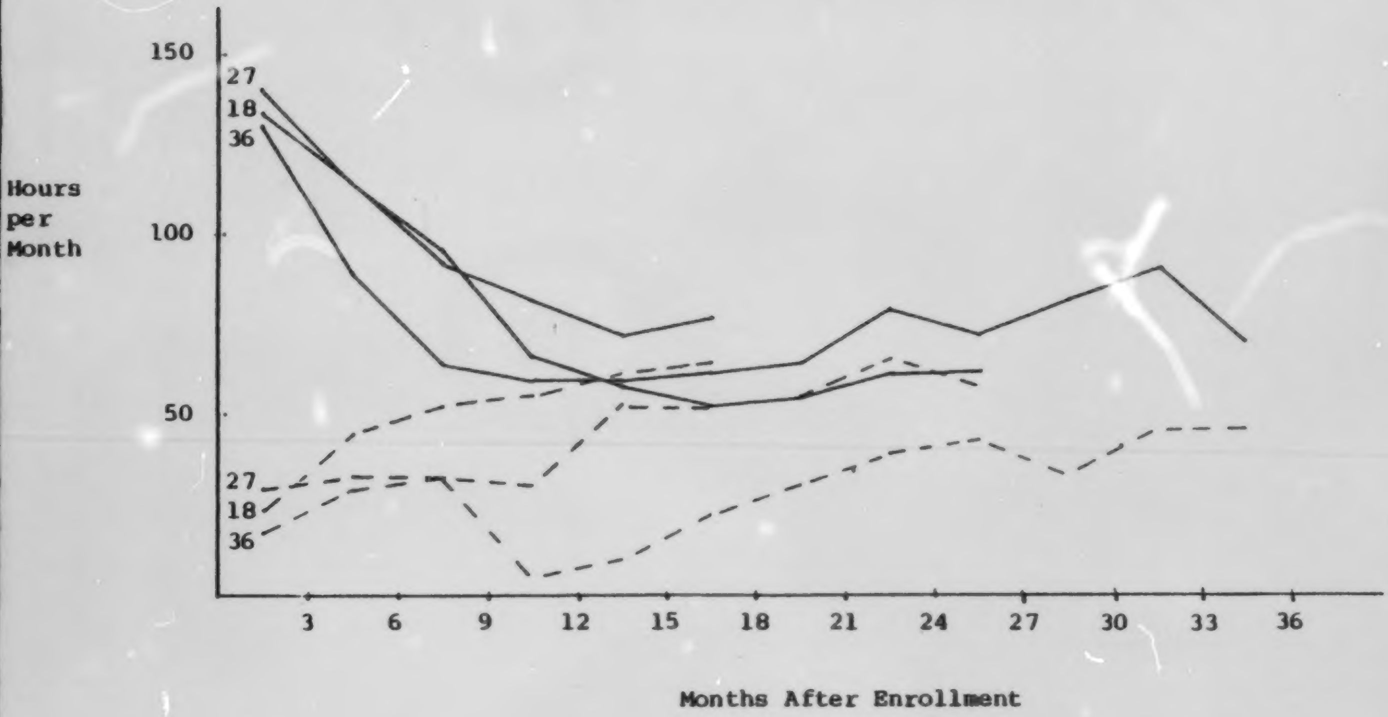
---

[1] These figures are the weighted average of the hours worked by those in the 27- and 36-month subgroups (e.g., .72 times the value for the 27-month subgroup plus .28 times that for the 36-month group).

FIGURE III.4

TREND IN HOURS WORKED PER MONTH BY SUBSAMPLES WITH VARYING AMOUNTS OF FOLLOW-UP DATA

(ATLANTA, HARTFORD, NEW YORK, AND PHILADELPHIA SAMPLES)



NOTE: These data are not regression-adjusted.

KEY: ________ Experimentals
_ _ _ _ _ Controls

18 = 18 Months of Follow-up Data
27 = 27 Months of Follow-up Data
36 = 36 Months of Follow-up Data

TABLE III.6

EMPLOYMENT RATES, HOURS EMPLOYED PER MONTH, EARNINGS PER MONTH, AND HOURLY WAGE, BY LATEST FOLLOW-UP DATA

	Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Percentage Employed						
18 months of follow-up	7.1	62.8	n.a.	n.a.	n.a.	n.a.
27 months of follow-up	3.7	61.0	-0.8	63.0	n.a.	n.a.
36 months of follow-up	20.0**	49.6	3.3	56.0	8.1	65.9
Average Hours Per Month						
18 months of follow-up	8.4	65.6	n.a.	n.a.	n.a.	n.a.
27 months of follow-up	5.0	60.8	-0.4	71.7	n.a.	n.a.
36 months of follow-up	40.7**	26.5	2.1	59.4	7.2	81.4
Average Earnings Per Month (Dollars)						
18 months of follow-up	15.89	224.17	n.a.	n.a.	n.a.	n.a.
27 months of follow-up	33.99	196.86	19.76	255.29	n.a.	n.a.
36 months of follow-up	97.07**	99.24	-0.56	231.27	-34.39	336.33
Average Hourly Wage Rate (Dollars)[a]						
18 months of follow-up	-0.17	3.42	n.a.	n.a.	n.a.	n.a.
27 months of follow-up	0.27	3.24	0.30	3.56	n.a.	n.a.
36 months of follow-up	-0.83	3.74	-0.14	3.89	-0.72	4.13

NOTE: See note to Table III.3. Together, these samples include the same observations used to generate data reported in Tables III.3 to III.5. This total sample has been partitioned according to the most recent interview assigned and completed. Continuous data was not required for inclusion in a subsample, while this requirement was imposed for the samples used to plot Figure III.2.

Among those with data for months 10-18, 51 percent have 18 months of follow-up only, 34 percent have 27 months, and 15 percent have 36 months. Among those with data for months 19-27, 28 percent also have data for 36 months.

[a] Wage rates have been computed by dividing average monthly earnings by average monthly hours. Thus, no significance levels are indicated.

**Statistically significant at the 5 percent level.

working in non-program jobs during the preceding period ($3.83 versus $3.11 per hour), while those with only 27 months of data earned roughly the same wage rates during both periods ($3.80 versus $3.86 per hour).[1/] Thus, the decrease in earnings differentials relative to hours differentials between experimentals and controls during this period was substantially larger for the 36- than for the 27-month subsample.

As noted earlier, among the 36-month sample, the upward trend in employment among both experimentals and controls between the 19- to 27- and the 28- to 36-month periods was due largely to the behavior of Jersey City sample members and was stronger among experimentals than among controls: experimentals increased the average number of hours they worked per month from 61 to 89 between these two periods, while controls increased their employment from 59 to 81 hours per month. However, concurrent with this increase in experimentals' employment, their average hourly wage fell by 34 cents per hour, while the smaller increase in employment among controls was accompanied by a 6 percent increase in hourly wage rates. The net result of these shifts is the observed negative earnings differential between experimentals and controls.

Based on the comparisons of the behavior of this sample with that of the samples followed for shorter periods, we have little reason to expect that a similar pattern of the employment results would necessarily have been observed for the full sample. In fact, it seems unlikely that the inverse relationship between employment and earnings differences would be exhibited by the full sample.

[1/] These wage rate figures are based on special calculations, and cannot be computed from data presented elsewhere in this report.

2. The Impact on Employment of Unemployment Compensation

Although the initial intention of the Supported Work design was not to have individuals become eligible for unemployment compensation (UC) on the basis of their Supported Work employment, a sizable portion of both the Jersey City and New York experimentals did receive benefits upon leaving the program.[1] For the full sample, then, there was a 6.4 point difference in the percentage of experimentals and controls who received UC during the 10- to 18-month period and a 3.5 point difference during the 19- to 27-month period.[2] However, the differentials were particularly large among the 36-month sample. This raises two questions, one being the extent to which this alternative income source reduced immediate post-program employment among experimentals, and the other being whether the pattern of employment and earnings differences for the samples with various amounts of follow-up data is due, in part, to the differential impact of UC.

Because a relatively small percentage of the total youth sample received UC benefits, their impact on overall program results has been estimated to be negligible.[3] Furthermore, even among those with 36 months of follow-up data, for whom the experimental-control differential in receipt rates was much larger (16 and 13 percentage points in the 10- to 18- and

---

[1] As previously noted, those in the Jersey City sample would have qualified under the SUA program, while the New York program participated in the state UC program.

[2] These differentials are considerably smaller than those estimated for either the AFDC or the ex-addict samples, but larger than for the ex-offender.

[3] For example, we have estimated that in the absence of any UC program the total hours differential during months 10 to 18 and 10 to 27 would have been only about 1.5 hours greater than those reported in Table III.6.

19- to 27-month periods, respectively), the estimated impact on employment results is relatively small,[1] and does not change the general conclusions regarding the generalizability of the 28- to 36-month results to the full sample.

3. The Importance of Public Sector Employment

As indicated in Chapter II, CETA employment potentially could be one of the main alternatives to Supported Work for this youth group. Thus, attempts were made to assess the extent to which the availability of such programs contributed to the observed employment differentials between experimentals and controls.

Overall, relatively few (between 1 and 8 percent) in either the experimental or control group reported employment in CETA or WIN jobs during any nine-month period, and experimental-control differentials in the prevalence of such jobs were low (-2 to +2 percentage points).[2] Yet, the differential in earnings from CETA and WIN jobs during the 28- to 36-month period is sizable and accounts for the large negative earnings differential: controls earned an average of 34 dollars per month more than did experimentals (see Table III.6), a differential that is equal to that in earnings from jobs identified by sample members as being CETA or WIN jobs.[3] Furthermore, to the extent that youth failed to distinguish between CETA jobs and other government jobs, the above figures understate the importance of subsidized employment, since the differential between experimentals' and controls' earnings from all government jobs, including CETA and WIN, is over 80 dollars per month.[1]

---

[1] For example, the hours differential during the 10- to 18-month period might be as much as 3 hours larger than estimated.

[2] See Appendix Table A.14.

[3] No attempt was made in the interviews to distinguish between CETA and WIN jobs. For the youth sample, however, most jobs will have been funded under CETA.

Thus, differential access to CETA, WIN, and other government jobs by those experimentals and controls with 36 months of follow-up data is related to the lack of significant longer-term employment impacts of Supported Work. However, this pattern of CETA, WIN, and government employment is peculiar to this sample subgroup and post-enrollment time period.[2]

4. Other Potential Explanations for the Observed Pattern of Results

It seems clear that the results estimated for months 28 to 36 on the basis of the 36-month sample are probably not representative of program effects one would expect to have observed if the full sample had been followed for that long. Furthermore, the unplanned availability of UC benefits to former Supported Work participants has not significantly affected the program's impacts. While differential access to public employment by experimentals and controls is related to the observed pattern of results, the reason for this relationship is not clear. In an effort to determine whether we could at least draw some conclusions as to the expected shift in the estimates of long-term impacts if the full sample had been observed, we considered several other issues. One of the more obvious ones was whether the pattern of results across the different subsamples reflected the changing site composition of the sample (see Table II.2). However, adjusting for the varying site compositions of the samples explained little of these differences. Other possible explanations for which we could identify no empirical support concern extreme values of the employment-outcome measures and differential nonresponse to interviews.[1] Another potential source of these differences is an interaction between the sites and local labor-market conditions. However, efforts to disentangle the labor-market and site influences have been plagued by a combination of the small sample sizes and the small variation in labor-market conditions across time relative to the variation across sites.[2]

---

[1] See Appendix Table A.14. As noted later in discussions of site-specific results, these differentials in CETA, WIN, and other public-sector employment between experimentals and controls are due to relatively high rates of employment in and earnings from such employment among control group members in Hartford and Jersey City. As noted in Table II.6, CETA jobs targeted at youth were relatively more prevalent in these two sites than they were in other sites.

[2] See Appendix Table A.15.

While we cannot fully explain why these subgroups of the youth sample behave differently, it is apparent that the long-term follow-up results reported here may not be indicative of the overall results that would have been observed for the full sample. Furthermore, because of the peculiar pattern of outcome measures across the different analysis samples and across different follow-up periods, it is not even possible to infer the nature of the expected shift in results that would have occurred from months 19 to 27 to months 28 to 36 if all sample youth had been interviewed at these later periods.[1] In drawing policy conclusions, therefore, little weight should be given to the results for the 28- to 36-month period.

---

[1] We found no extreme values for either hours or earnings (e.g., earnings in excess of $7 per hour and hours in excess of 70 per week) (see Table A.10).

Brown (1979) and Appendix B provide evidence to suggest that interview nonresponse has not resulted in biased estimates of program impacts.

[2] We should remind the reader that the results discussed above are regression-adjusted, so differences across analysis samples should not be due to variations in the measured characteristics of the different groups. (See Table II.4.)

D. DIFFERENTIAL IMPACTS ON SAMPLE SUBGROUPS

The overall sample results discussed in the preceding section provide no evidence to suggest that Supported Work is successful in its goal of improving youth's long-term employment experiences. We may be able to gain some insights as to possible reasons for this by considering the extent to which program impacts vary across sample subgroups.[2]

1. Differential Impacts Across Sites

Table III.7 presents estimated program impacts on hours worked for each of the five Supported Work sites enrolling youth.[3] Differentials in impacts during the first nine months, when all experimental group

---

[1] Given that experimental-control differentials appear to be strongly related to control behavior rather than experimental, an unsubstantiated judgment is that the full sample would exhibit smaller employment-rate and hours differentials and more positive earnings differentials than those estimated for the small subsample who completed a 36-month interview. (The estimated program impacts for other target groups also suggest that Supported Work may have sustained impacts only among individuals for whom or during periods when alternative employment opportunities are extremely limited.)

[2] Sample sizes for the various subgroups can be determined by multiplying the proportion of the sample in each subgroup by the overall sample size. These proportions and sample sizes are presented in Appendix Table A.4a.

[3] Site impacts were also estimated using tobit, with similar results, except that the 28- to 36-month differential for the Philadelphia sample is significant at the 10 percent level. (Tobit estimates are presented in Appendix Table A.16.)

TABLE III.7

HOURS WORKED PER MONTH, BY SITE

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth[a]	80.7**	39.7	11.7**	58.2	0.6	68.2	7.2	81.4
Site	#							
Atlanta	77.1**	59.1	5.5	79.2	-15.5[b]	127.9[b]	n.a.	n.a.
Hartford	87.2**	35.1	11.9*	49.7	3.9	62.5	9.6[b]	81.1[b]
Jersey City	94.6**	45.1	-0.2	73.1	-11.2	85.8	-10.5	102.7
New York	61.5**	39.9	22.5*	61.8	21.2[b]	32.8[b]	n.a.	n.a.
Philadelphia	46.9**	31.1	30.7*	30.8	14.3	45.3	35.9	44.6

NOTE: See note to Table III.3. Samples sizes for various subgroups can be calculated by multiplying the proportion of the sample in the subgroup by the total sample size. These figures are presented in Appendix Table A.4.

[a] These overall sample results were estimated from an equation that did not include variables interacting experimental status with site. Thus, the subgroup results may not weight up to exactly the overall sample results.

[b] These data are based on sample sizes of 20 or less.

Experimental-control differentials vary significantly among the sites. (Section II.E describes the test procedure.)

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

n.a. = not applicable

members were eligible to hold program jobs, are due almost entirely to variation in the average length of time experimentals spent in Supported Work: in Jersey City, where the average length of stay was 8.2 months, experimentals worked 95 hours more per month than controls; in Philadelphia, where experimentals stayed in the program only 3.7 months on average, experimentals worked only 47 hours more per month than controls.

In the subsequent time periods, as success of experimentals in non-program jobs became increasingly more relevant, the pattern of estimated differentials across sites looks quite different. The point estimates of the experimental-control differences are always negative (though not significantly different from zero) for the Jersey City sample, and they are always positive for the Hartford, New York, and Philadelphia samples. However, only the 12-, 23-, and 31-hour differentials estimated for the the Hartford, New York, and Philadelphia samples, respectively, in the 10- to 18-month period are statistically significant.[1] Unlike the first 9-month period, the differential impacts in these later periods were attributable primarily to the high variance across sites in employment among controls. For example, in each time period, controls in Jersey City worked 25 percent more than the average for all controls, while those in Philadelphia worked only 53 to 66 percent as many hours per month as the average for all controls. Experimentals' hours also tended to be relatively high in Jersey City and Atlanta and relatively low in the other three sites, but the range was considerably smaller than observed among the various control groups.

---

[1] Furthermore, the average of the experimental-control differential for the Hartford, New York, and Philadelphia sites was significant only for months 1 to 9 and 10 to 18. The estimated program effect was significantly different between those enrolled in Hartford, New York, and Philadelphia, and those enrolled in Jersey City or Atlanta only in months 1 to 9.

Thus, as in the case of the differential impacts among samples with different amounts of follow-up data, we observe that a more favorable pattern of estimated program impacts was observed among those youth who otherwise had extremely limited employment opportunities, as evidenced by the controls' behavior. What is not apparent from this pattern of results is the cause of the relatively low employment among controls in New York, Philadelphia, and Hartford. On the one hand, the previously noted trends in area unemployment rates (see Figure II.1) are not consistent with this pattern of employment among controls in the various sites. The allocation of CETA employment opportunities for youth (see Table II.6), on the other hand, does suggest a relatively lower rate of public-employment opportunities for control youths in New York and Philadelphia as compared with those in other sites. While the controls' reports of CETA employment indicate that employment in CETA jobs per se contributed relatively little to the cross-site variation in their employment, 17 to 30 percent of the controls in Atlanta and Jersey City reported having jobs in the public sector.[1] Also, as noted previously, differential access to CETA and other public-sector employment by experimentals and controls who were interviewed 36 months after enrollment is related to the pattern of results observed for the 28- to 36-month period, particularly those results pertaining to earnings.

Other factors that may affect the net impact of Supported Work in the various sites by influencing employment among experimentals are the

---

[1] Appendix Tables A.17 and A.18 display the percentage of experimentals and controls with CETA or WIN jobs and the percentage with CETA, WIN, or other government jobs.

Supported Work programs themselves and the receipt of unemployment compensation. As noted in Chapter II, these five Supported Work programs vary along so many dimensions that it was not possible to assess the impact of various program characteristics on subsequent employment experiences of experimentals or on net program impacts.[1/] Estimates of the effects of unemployment compensation programs suggest that these programs, at most, had the effect of resulting in negative observed differentials between experimentals' and controls' hours when the differential would otherwise have been expected to be zero (e.g., among the Jersey City sample), and of dampening the positive differentials observed for the New York sample.[2/]

2. Differential Impacts Among Subgroups of the Youth Sample

Table III.8 presents estimates of program impacts on hours worked for various subgroups identified by demographic and background characteristics, in order to determine whether focusing the program on a somewhat different group of youth than that represented by this evaluation sample would be appropriate. In general, there is no evidence from this subgroup

---

[1/] Hollister et al. (1979) summarize the results of an analysis--based on the 13 Supported Work programs initially included in the national demonstration--of the effects of program, project, and supervisor characteristics on various outcome measures. They find some evidence that the type of work project, the type of supervision, and the extent of supportive services are related to program impact. However, as the authors point out, these findings may be subject to selection bias.

We considered whether the length of time a site had been operating affected the program's impact, but found no significant relationship. In addition, we considered the possibility that the Manpower Demonstration Research Corporation (MDRC), which manages the demonstration, could identify operational strengths in various programs that correlated with the impacts on youth, but determined that such strengths were not evident.

[2/] Supporting data are presented in Appendix Table A.19.

TABLE III.8

HOURS WORKED PER MONTH, BY DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth[a]	80.7**	39.7	11.7**	58.2	0.6	68.2	7.2	81.4
Years of Age								
Under 19	82.7**	38.4	13.3**	57.9	5.5	60.9	11.5	77.0
19 or older	76.9**	41.7	8.7	58.0	-5.8	78.2	-4.4	95.7
Sex	#							
Male	76.7**	42.3	12.6**	59.7	1.9	72.2	1.7	87.4
Female	103.4**	24.0	4.0	46.9	-8.2	34.0	52.0[c]	47.7[c]
Race/Ethnicity			#					
White, not Hispanic	90.2**	38.8	54.1**	51.5	-32.1	109.5	50.4[c]	87.4[c]
Black, not Hispanic	80.5**	37.9	8.7	54.9	2.9	61.3	3.5	80.9
Hispanic	75.6**	49.6	8.9	75.9	10.6	77.1	-14.6[c]	111.3[c]
Years of Education								
8 or less	78.7**	38.3	6.1	51.4	-18.6	64.8	43.1	55.3
9 or more	80.7**	40.0	12.4**	59.1	4.7	68.7	-3.9	91.7
Time Since Last Enrolled in School[b]								
Less than one year	71.7**	44.5	20.4**	57.8	4.2	61.5	-1.5	93.3
One to two years	88.8**	36.0	10.4	61.4	-9.1	78.6	52.8*	50.7
More than two years	85.2**	37.6	2.8	56.3	5.4	65.8	-11.1	89.1
Reason Left School[b]					#			
Expelled	78.1**	36.5	-1.4	57.9	-16.9	73.4	-6.6[c]	67.9[c]
Jailed or in trouble with police	76.6**	33.4	9.3	54.5	21.5	42.9	15.9	93.6
Wanted a job	84.1**	45.0	18.0**	63.0	15.6	74.7	-8.2	97.2
Other	81.9**	39.5	11.9	56.3	-13.3	73.0	28.0	59.8
Living Situation at Baseline[b]	#				#			
With parent(s)	85.7**	36.1	11.2*	53.2	-8.9	72.4	17.5	70.0
Not with parents	70.9**	48.2	11.3	70.0	25.0**	57.5	-22.3	117.7

TABLE III.8 (continued)

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Raised by[b/]								
One parent	75.7**	40.2	4.4	64.6	-0.5	66.0	5.5	97.8
Two parents	91.3**	37.8	22.8**	50.5	1.8	66.6	13.6	64.0
Other	76.9**	43.9	9.7	48.0	2.2	94.0	-26.1	94.5
Welfare and Food Stamp Receipt in Month Prior to Enrollment[c/]								
None	77.6**	40.8	7.9	57.3	6.0	70.9	-6.4	93.0
Some	85.7**	37.8	18.3*	59.1	-11.1	61.6	37.2	61.7
Dependents					8			
None	82.3**	38.7	9.4*	59.7	-3.1	71.3	5.8	81.8
One or more	63.6**	49.1	29.4*	42.1	42.3*	33.5	-13.4[e/]	138.1[e/]
Months in Longest Job								
0	73.1**	38.9	0.6	56.7	-19.1	77.6	49.3	39.6
1 - 12	81.9**	40.3	15.7**	56.7	10.4	62.3	-13.1	103.4
More than 12	88.3**	37.1	5.4	72.8	-23.6	90.5	31.7[e/]	55.8[e/]
Weeks Worked in Year Prior to Enrollment[d/]								
0	80.5**	36.4	15.8**	51.5	0.2	61.6	20.4	69.8
5	80.4**	38.2	13.5**	54.9	0.5	64.8	14.2	75.8
10	80.4**	40.0	11.1**	58.4	0.8	68.0	8.0	81.9
Weeks of Job Training in Year Prior to Enrollment	8						8	
Less than 8	78.1**	39.6	9.6*	59.1	-0.4	68.8	18.2	75.0
8 or more	99.1**	41.0	26.6*	48.4	10.7	62.2	-78.9*	147.1
Prior Drug Use .9								
Used drugs other than marijuana	73.3**	47.4	15.5	47.6	16.3	57.6	10.8	84.0
Did not use any drug other than marijuana	82.5**	37.4	10.2*	61.1	-5.8	72.5	0.4	85.8
Prior Arrests[d/]								
0	79.0**	42.9	17.8**	61.0	-5.0	81.8	12.8	88.5
4	85.0**	38.6	8.1	57.6	10.8	57.2	6.7	87.0
9	79.2**	38.9	3.9	58.0	9.6	58.0	4.0	91.8

TABLE III.8 (continued)

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Months Since Incarceration	#				#			
Never incarcerated	84.3**	41.6	13.7**	60.6	12.4	62.9	15.7	93.2
12 or less	62.6**	35.4	4.9	43.8	12.2	41.3	18.9	43.9
More than 12	94.5**	32.0	-1.9	62.3	-26.1*	88.6	-20.2	103.9

NOTE: See note to table III.1.

[a] These results were estimated from an equation that included only the standard control variables and an experimental-status variable. Thus, the subgroup results may not always weight up to those overall sample values.

[b] These subgroup results were estimated in separate regressions which did not simultaneously control for potential differences among those with other characteristics.

[c] Welfare includes AFDC, General Assistance, other welfare, and welfare income for which respondents could not identify the source.

[d] These estimates of subgroup effects and means are based on a linear (or piecewise linear) specification of the sample characteristic, evaluated at the specified points.

[e] These data are based on sample sizes of 20 or less.

#Experimental-control differentials vary significantly among these subgroups. (Section II.E describes the test procedure.)

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

analysis that Supported Work will mitigate the employment problems of any subset of the youth population. The results are no more than suggestive that a program of this type *may* be more successful if targeted at younger youths, those raised in intact families, those who have one or more dependents to support, those who have little or no recent work experience, and those who have used drugs (other than marijuana or alcohol). Each of these subgroups is characterized by relatively low employment among control group members, as were the sites where more positive experimental-control differentials were observed. The point estimates of experimental-control differentials for these various subgroups tend to be consistently positive and larger than for the overall sample, but they are infrequently significantly different from zero, except for months 1 to 18.[1] Furthermore, the variance across subgroups in the point estimates of program impacts is sufficiently large that, generally, we could not conclude that the point estimates are significantly different from each other.

E. PATTERNS OF NON-PROGRAM EMPLOYMENT

It is possible that while not affecting the more obvious measures of success--employment rates, hours worked, and earnings--Supported Work might favorably influence the pattern of youths' employment or the nature of their jobs. Furthermore, an examination of the non-program employment experiences of experimentals and controls might shed some light on the reasons why the program had so little impact along these main dimensions.

---

[1] We considered the possibility that individuals who had more than one of the characteristics might constitute an appropriate target group for a Supported Work program. While we found no evidence to suggest that significant program effects would be observed for these subsamples, the sample sizes for this analysis were generally very small.

Table III.9 presents various data describing the non-program employment experiences of experimental and control group members for whom various amounts of follow-up data are available. In considering these data, it is important to remember that experimental group members spent an average of six to seven months in their first spell of Supported Work (13 percent returned to the program one or more times after initial termination). Therefore, the data compare the employment experiences of experimentals and controls over different lengths of time: for those with 18 months of follow-up data, the reference period for experimentals averaged only 11 months as compared with 18 months for controls; similarly, for the 27-month and 36-month follow-up samples, the reference periods for experimentals averaged 21 and 28 months, respectively. As a result of these different reference periods for experimentals and controls, there will be a tendency for experimentals' experiences to look slightly less favorable than controls', particularly among the sample for whom only 18 months of follow-up data were available.

Indeed, what we observe is that the non-program employment experiences of the two groups are quite similar. For all three samples, a slightly lower percentage of experimentals than controls had some non-program job (mainly reflecting their shorter reference period), and experimentals worked only a slightly higher percentage of the available weeks than did controls.[1/] Among both experimentals and controls, two-thirds to three-quarters of the jobs were in the manufacturing, retail

---

[1/] For experimentals, their available weeks are considered to be those since their first Supported Work termination, and for controls, they include the full follow-up period.

TABLE III.9

NON-SUPPORTED-WORK EMPLOYMENT EXPERIENCE

YOUTH SAMPLE

	Sample with 18 Months of Follow-up Data		Sample with 27 Months of Follow-up Data		Sample with 36 Months of Follow-up Data	
	Experimentals	Controls	Experimentals	Controls	Experimentals	Controls
Month of First Supported Work Termination[a]	6.4	n.a.	6.4	n.a.	7.2	n.a.
Percentage with Non-Supported Work Employment	65.8	78.4	74.8	82.8	90.3	93.2
Of Those with Non-Supported Work Employment,						
Percentage who found job through						
Supported Work	15.3	n.a.	10.6	n.a.	5.4	n.a.
Employment Service	3.1	8.3	8.7	13.1	7.1	21.8
Percentage with rollover jobs[b]	3.8	n.a.	2.9	n.a.	1.8	n.a.
Percentage with CETA or WIN jobs	9.2	8.8	11.5	13.7	16.1	16.4
Percentage with CETA, WIN, or government jobs	23.7	23.2	26.0	29.8	33.9	50.9
Hours worked per week[c]	18.8	15.4	17.4	14.7	16.1	14.6
Hours worked per week when worked[c]	39.3	38.4	38.8	38.8	35.8	39.3
Wage per hour (dollars)[d]	3.44	3.40	3.76	3.41	3.43	3.59
Length of first continuous spell of employment (months)	3.9	5.0	6.0	5.6	5.8	4.7
Percent in their first job at end of follow-up period	32.3	26.5	25.0	13.8	19.6	5.5
Number of spells of employment	1.4	1.5	1.7	1.9	2.2	2.6
Percentage of available weeks employed	48.3	38.9	43.9	36.8	42.9	38.1

NOTE: These data are not regression-adjusted. Samples used include only those observations for whom continuous data for the indicated length of time (18, 27, or 36 months) were available. Data pertain to the full period covered by the interview data.

[a] Thirteen percent of the sample left the program more than once. On average, individuals were in Supported Work 6.3 months at the time of their first termination. The overall average length of stay was 6.7 months.

[b] A participant with a rollover job is one who has the same job as during Supported Work participation, but whose wage is no longer subsidized by Supported Work nor does the Supported Work program provide supervision.

[c] For experimentals, the average hours worked per week were calculated for the period since leaving Supported Work. They do not include non-Supported-Work hours during the period of program participation.

[d] These wage rates are calculated as the average, for all individuals who had jobs, of their total earnings divided by the number of hours worked.

trade, and service industries, and they were mainly in clerical, service, and miscellaneous occupations.[1]

A surprisingly small percentage (less than 20) of the sample reported that the Supported Work program or the employment service had helped them find their jobs, and less than 3 percent of the experimentals' non-program jobs were a continuation of their Supported Work jobs with a shift to alternative funding and supervision (i.e., rollover jobs). The percentage of experimental youth who reported that the Supported Work program had helped them find a non-program job increased over time, from 3 percent of those enrolled in 1975 to 7 percent of those enrolled in 1977. Also, youth in Hartford and Atlanta were much more likely to report program assistance (9 and 8 percent, respectively) than those in the other sites (less than 5 percent in Jersey City and New York and none in Philadelphia).

CETA jobs were much less prevalent among both groups than we had anticipated they might be, and there was little difference in the prevalence of such jobs between experimentals and controls who were employed. Between 9 and 16 percent of the different subsamples reported such jobs. However, two factors are noteworthy: the first is that the average income from CETA employment increased over calendar time,[2] and the second is that CETA employment tended to be more prevalent among the Jersey City controls than among controls in other sites or than among experimentals in this site. (These facts may partially explain the relatively high employment among controls in the 28- to 36-month period, which on average is 3 months later than the calendar time covered by the 19- to 27-month results, and which includes a sample of which 56 percent are from Jersey City.)

---

[1] See Appendix Tables A.20 and A.21.

[2] For example, for the full sample, CETA earnings during late 1976 and early 1977 averaged about $5 per month; during late 1977 the average was $10 per month; and during late 1978 it was about $17 per month.

In terms of other aspects of the non-program jobs, there are no noteworthy differences. Among those who were employed, the average hours worked per week when employed was 36 to 39, suggesting that nearly all worked at full-time jobs. Experimentals worked slightly more hours per month than controls; but for the full youth sample, the average hours worked in non-program jobs per "available" month was about the same for both groups (11 to 15). Average hourly wage rates varied between experimentals and controls by 5 to 10 percent in either direction. Among the 36-month sample, experimentals exhibited a tendency toward more stable employment--the average length of their first non-program job being 6 months, as compared with 5 months for controls (not accounting for the fact that a higher percentage of this group than of controls were still in that first job at the time of their final interview--20 versus 6 percent).[1]

As we noted in the discussion of experimentals' Supported Work experiences, youth tended to stay in Supported Work longer than they stayed in other types of jobs (6.7 months, as compared with 5 to 6 months for non-program jobs).[2] Yet, neither for Supported Work jobs nor for non-program jobs has longer tenure been found to result in improvements in other dimensions of employment-related outcomes such as employment rates or levels.

---

[1] Of those youth who left their non-program jobs, 30 to 40 percent expressed dissatisfaction with the job. Half of the experimentals and 36 percent of the controls reported having left due to lack of work. Ten percent of the experimentals and 6 percent of the controls left for a better opportunity (see Appendix Table A.22).

[2] While the data in Table III.9 indicate that an average spell of employment among controls lasted between 4.7 and 5.6 months, recall that some controls were still in their first job at the end of the period covered by the interview data.

F. IMPACTS ON EMPLOYMENT STATUS AND JOB SEARCH

In addition to directly altering employment opportunities for youth, it was expected that Supported Work might increase youths' participation in the labor force and alter the extent and nature of their job-search activity. However, except for the first nine months following enrollment, when a significantly higher percentage of experimentals than controls were employed in Supported Work and so not actively engaged in job search, there was little difference in either the labor-force status or job-search activity of the two groups. After month 9, between half and three-quarters of these youth were in the labor force during a given month, and about half of those in the labor force were employed. Those looking for work spent an average of about eight hours per week in search activities, which included an average of five to six contacts with employers.[1] Less than half the youth looking for work reported checking with the state employment service and only a few (3 to 14 percent) checked with the CETA office. Most efforts appeared to have involved less formal search methods such as contacting friends, looking in the newspaper, and checking with employers directly.

We do observe a consistent pattern of higher reservation wage rates[2] among experimentals, which are between $5 and $15 per week (4 to

---

[1] The above data refer to job-search activity during the four weeks preceding each follow-up interview (see Appendix Table A.23).

[2] The reservation wage rate is the lowest wage for which the individual is willing to work.

14 percent) higher than among controls.[1] It is possible, then, that this higher reservation wage among experimentals has led to the discouraging results with respect to job-search efforts, which resulted in both lower employment rates and reduced labor-force participation than would otherwise have been expected. However, we have not undertaken a formal test of this notion.[2]

This lack of program impact on job search suggests that there will not be employment effects in later time periods than covered by our data which are attributable to program-induced changes in job-search activity.

G. IMPACTS ON EDUCATION AND TRAINING

Limited education and formal training are often cited as one of the main reasons for the employment problems of youth. While the Supported Work program itself emphasizes the provision of work experience rather than formal training, there are reasons to expect that it might nonetheless affect the education and training decisions of participants and former participants. On the one hand, Supported Work might increase participation in such programs by altering attitudes towards, providing information about, or providing an income source to support investments in human capital. On the other hand, the program may tend to limit such investments by directly increasing employment opportunities, particularly in the short-run, and consequently the opportunity costs of such investments.

---

[1] This pattern of differentials between experimentals' and controls' reservation wage rates does not appear to be related to differentials in unemployment compensation receipt (see Table IV.1a and Appendix Table A.23).

[2] Employed controls consistently had higher average reservation wage rates than experimentals and than the unemployed or nonparticipants in the labor force. This suggests that more experimentals probably could have found work than did so, but at lower wage rates than the average commanded by those who had found employment.

While the differences were not statistically significant, a substantially higher percentage of experimentals than controls reported that they had not undertaken any formal job search during the 10- to 18-month period (14 versus 10 percent) and the 19- to 27-month period (16 versus 10 percent) because they felt that they would not be able to find work.

As can be seen from Table III.10, Supported Work tended to reduce slightly alternative investments in human capital during the first 18 months following enrollment (particularly while experimental group members were participating in Supported Work), but not thereafter. However, education and training by both groups was quite limited throughout the follow-up period: 6 to 16 percent were enrolled in education programs (mainly high school) during any 9-month period, and less than 10 percent were in formal training programs (a third or less of which were sponsored by CETA).

H. CONCLUSIONS

The weight of the evidence suggests that Supported Work is not an effective means of mitigating the employment problems faced by youth. Youth stayed in Supported Work jobs for considerably shorter periods of time than permitted under program regulations, yet less than 20 percent of them left to take another job. Over 40 percent left, indeed, for negative reasons. At some time subsequent to leaving Supported Work, most of the youth in the experimental group did find non-program employment: about 30 percent did so within the first month after leaving, and two-thirds had done so within one year of leaving. However, what we also observe is that many of these youth would have been similarly successful in finding

TABLE III.10

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Education Programs								
Percentage participating	-3.5	15.8	0.9	10.1	1.7	9.0	-3.7	9.0
Average number of weeks	-1.3**	2.9	-0.1	1.8	-0.1	1.8	-0.8	1.7
Of those participating[a]								
Percentage in high school program	1.4	68.9	-10.8	75.7	10.0	60.0	0.0	50.0
Percentage in vocational program[b]	-4.7	15.6	8.1	10.8	1.7	13.3	0.0	0.0
Percentage in college program	1.9	8.9	-5.4	13.5	-21.7	26.7	16.7	33.3
Percentage in other program	1.5	6.7	8.1	0.0	10.0	0.0	-16.7	16.7
Percentage receiving diploma or degree[a]	0.1	1.2	-0.6	2.0	1.4	0.8	-2.6	2.6
Training Programs								
Percentage participating	-3.7**	7.2	-4.0**	9.6	-0.2	5.7	-0.5	4.2
Average number of weeks	-0.6**	1.1	-0.7*	1.6	-0.2	1.3	-0.0	0.6
Of those participating, percentage in programs sponsored by[a]								
Supported Work	33.3**	0.0	0.0	0.0	0.0	0.0	0.0	0.0
CETA or WIN	-13.3	26.7	2.2	34.1	21.4	21.4	-33.3	33.3
Jail or prison	-6.7	13.3	-17.7*	26.8	-14.3	35.7	0.0	33.3
Other	-13.3	60.0	15.5	39.0	-7.1	42.9	33.3	33.3
Percentage receiving certificate[a]	-1.0	2.5	-4.3**	5.8	0.7	2.2	-2.6	2.6

NOTE: See note to Table III.3. Unless otherwise noted, data pertain to the full sample.

[a] These data are not regression-adjusted.

[b] These figures may include those for vocational high school programs.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

employment had they not participated in Supported Work, as evidenced by the employment rates of the control group; these rates, while much lower than the average for all youth, are higher than for other Supported Work target groups and than were expected for this subgroup of youth on whom Supported Work focused. The analysis of differential impacts across sites and subgroups of youth show a consistent (though generally not significant) pattern of relatively more favorable program effects among those youth whose control group counterparts exhibit unusually low levels of employment: the earliest enrollees in the demonstration; those in New York, Hartford, and Philadelphia; and those who are younger, have one or more dependents, were raised by two parents, and have little or no recent employment experience and some history of drugs. However, the results provide no strong evidence to suggest that simply redirecting Supported Work to focus on this subset of youth would substantially alter the conclusions concerning its success in its primary objective of improving youth's long-term employment prospects.

The program clearly does have short-term benefits in terms of increasing the employment opportunities for this segment of the population. However, in judging whether these immediate benefits can justify the continuation of Supported Work programs for youth, one must give careful consideration to its cost-effectiveness in relation to alternative programs.[1/]

[1/] See Kemper, Thornton, and Long (1980) for a detailed discussion of the costs of Supported Work, the value of its benefits, and the relationship between the costs and benefits of Supported Work and those of alternative employment programs.

CHAPTER IV

INCOME, IN-KIND TRANSFERS, AND RELATED OUTCOMES

One rationale for public expenditures on employment and training programs for youth is that they will increase the economic status and independence of the participants, both while they are enrolled in the program and subsequently. During the month prior to enrollment in Supported Work, the youth sample exhibited a very low level of income (about $100), 35 percent of which came from public assistance. In this chapter, we consider the short- and longer-run impacts of Supported Work on the total income received by participants and the sources of this income. In addition to being concerned with overall economic status, we are interested in the extent to which individuals become relatively more dependent on earnings and less so on transfer income and various forms of in-kind assistance,[1] and the extent to which program-induced changes in earnings and other sources of income might lead to significant changes in other areas, such as household composition, expenditures for housing, and medical-care utilization.

---

[1] During each 9-month period, 3 to 17 percent of the experimental and control youth reported receiving some money through illegal activities (mainly theft and selling drugs). However, because the data are of questionable accuracy, because the differentials between experimentals and controls tend to be small, and because the amount of income is small in relation to income from other sources (less than 10 percent), we have not included illegal activities among the sources of income considered in this chapter.

In the next section we discuss the effect of Supported Work on receipt of income from each of five different sources: earnings, unemployment compensation, welfare, food stamp bonuses, and other programs or persons.[1] Subsequently, we consider changes in receipt of in-kind benefits. These various sources of income and in-kind benefits are depicted in Figure IV.1. The third section discusses results for a variety of outcomes related to economic well-being.

A. PROGRAM EFFECTS ON INCOME

With the exception of the first nine months after enrollment, when, as a result of their Supported Work jobs, experimentals had substantially higher earnings than controls, program impacts on both total income and its sources were modest and generally not significantly different from zero. As seen from Table IV.1, during the first 18 months following enrollment, experimentals were more likely than controls to be employed (mainly in Supported Work jobs) and their resulting higher earnings led to significant reductions

---

[1] Since 65 percent of the sample youth lived with their parents at the time of enrollment in the demonstration, we might expect the youth's income gains to particularly influence the transfer income received by other household members. The only evidence we have of such an effect, however, is a reported six-point reduction in the percentage of experimentals, relative to controls, who reported that other household members received any form of public assistance during the first 9 months following enrollment. This result does not persist into subsequent periods; furthermore, it may be confounded with trends over time in the numbers of youth who are living with their parents.

FIGURE IV.1

CATEGORIES OF INCOME AND IN-KIND BENEFITS USED IN THE ANALYSIS

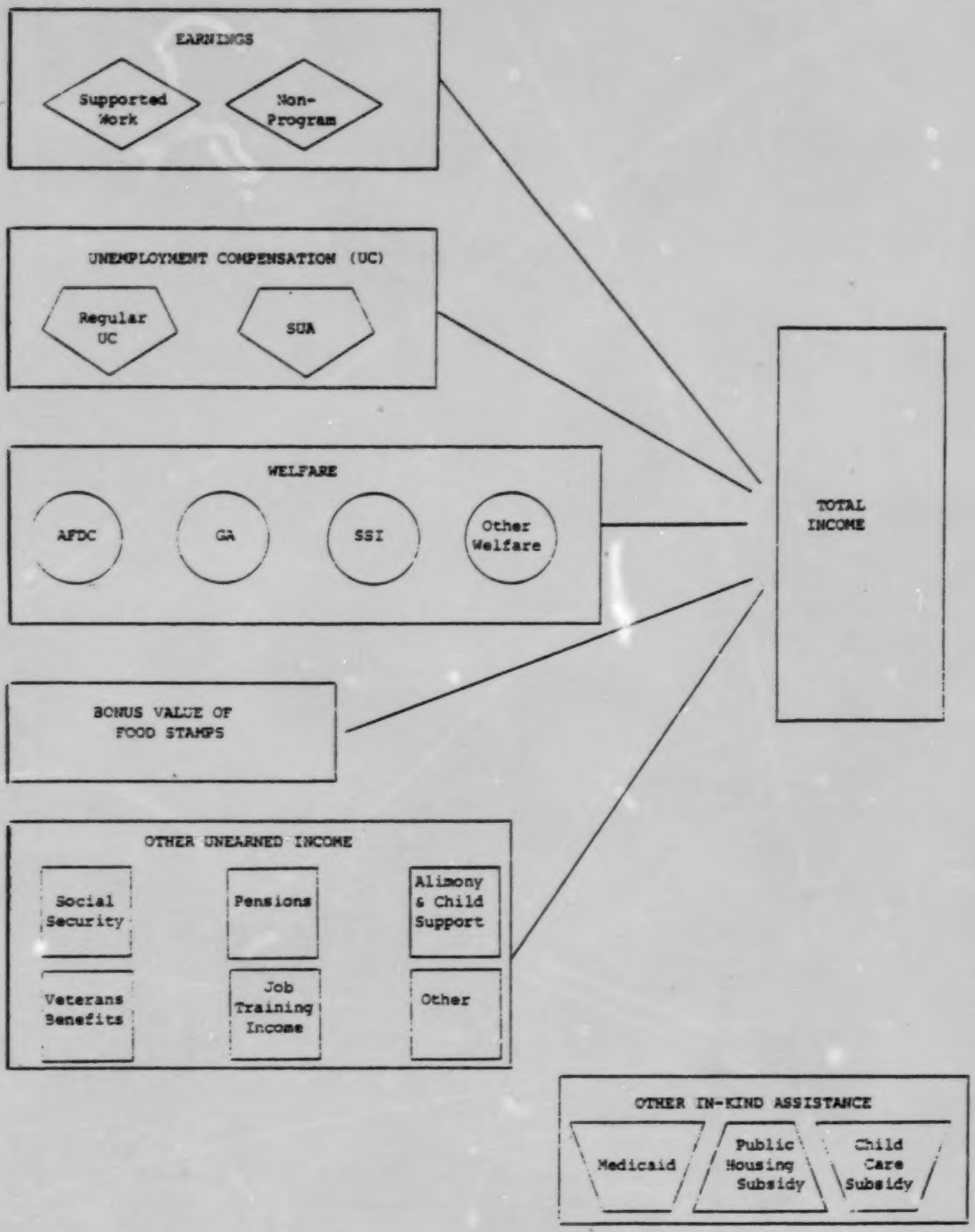

EARNINGS
Supported Work
Non-Program
UNEMPLOYMENT COMPENSATION (UC)
Regular UC
SUA
WELFARE
AFDC
GA
SSI
Other Welfare
BONUS VALUE OF FOOD STAMPS
OTHER UNEARNED INCOME
Social Security
Pensions
Alimony & Child Support
Veterans Benefits
Job Training Income
Other
TOTAL INCOME
OTHER IN-KIND ASSISTANCE
Medicaid
Public Housing Subsidy
Child Care Subsidy

TABLE IV.1a

PERCENTAGE RECEIVING INCOME FROM VARIOUS SOURCES

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Earnings [a]	45.6**	52.5	6.2*	62.7	0.0	62.6	8.7	66.2
Unearned Income								
Unemployment Compensation	-2.0	4.0	6.8**	3.7	3.8*	3.8	-5.6	6.8
Welfare [b]	-6.8**	17.0	-3.5	21.4	-1.3	20.6	-11.4*	25.0
Food Stamps	-0.5	32.4	-3.7	30.5	-0.6	29.0	6.6	30.3
Other [c]	-2.0	6.3	-1.1	5.1	0.4	3.1	0.7	1.7

TABLE IV.1b

INCOME RECEIVED PER MONTH FROM VARIOUS SOURCES

YOUTH SAMPLE

(dollars)

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Sources	215.01**	176.04	21.95	265.44	26.99	311.68	-54.54	408.01
Earnings [a]	226.73**	123.95	30.71	205.25	19.30	248.98	-41.53	342.58
Unearned Income								
Unemployment Compensation	-2.99	5.63	11.16**	4.71	10.14**	5.80	-8.16	10.25
Welfare [b]	-5.66**	22.85	-12.49**	33.00	-6.20	37.30	-15.75	36.01
Food Stamps	0.88	17.52	-3.63*	15.44	-1.42	16.82	5.18	18.78
Other [c]	0.94	5.65	-3.34	6.81	5.91	2.42	5.33	0.58

NOTE: See note to Table III.3. All data pertain to the full sample, not only to recipients.

[a] Earnings data reported in this chapter vary somewhat from those reported in Chapter III, because of a slight difference in the samples used; only individuals who have valid data for all income sources listed in this table were included in the analysis reported here.

[b] Welfare includes AFDC, GA, SSI, and other welfare income for which respondents were unable to identify the source.

[c] Other unearned income includes Social Security, Pensions, alimony, child support, and job-training income.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

in welfare benefits[1] and food stamp bonuses, averaging about $12 per month.[2] However, during months 10 to 18, the reduction in public-assistance benefits was largely offset by a significant $11 per month increase in unemployment compensation (UC) among experimentals relative to controls.[3] By months 19 to 27, the only significant differentials were in the percentages receiving and the average value of UC benefits. However, these differences are relatively small (4 percentage points and $10 per month, respectively) and, as noted previously, they are concentrated among the Jersey City sample.

During months 28 to 36, the overall effects indicate a curious pattern of increased employment, decreased reliance on transfer programs, and an overall reduction in total income (due to lower earnings) among experimentals relative to controls. However, the only differential that is statistically significant is the 11 percentage-point reduction in receipt of any welfare benefits.

---

[1] For a subset of the Supported Work AFDC sample, interview data on welfare receipt were compared with welfare agency data. It was found that interview data understated by a small amount actual receipts and that the degree of misreporting was strongly related to changes in receipt. For the AFDC sample, it was estimated that misreporting may have led to as much as a 12 percent error in the results (Kerachsky et al., 1979). However, because of youth's lower receipt rate and incidence of changes in receipt, the likely effect of misreporting for them is very small.

[2] On average, females received two to three times as much income from welfare and food stamp bonuses as did males and, consequently, program impacts tended to be substantially higher for them since impacts were strongly related to preenrollment benefit levels. Among a number of other sample subgroups for whom program impacts were estimated, there was no consistent pattern of differential effects.

[3] This UC differential occurred mainly among the Jersey City sample, in which 40 percent of the experimentals versus 8 percent of the controls received benefits.

The trends in total income and income sources of the entire sample are depicted in Figure IV.2. Except for the sharp increase in experimentals' income during their participation in Supported Work, experimental and control youth experienced roughly similar increases in their total incomes over time (from about $120 per month prior to enrollment to between $350 and $400 per month during the third year following their program enrollment). This trend is attributable almost entirely to a general increase in employment and earnings, which, in the long run, does not appear to be significantly affected by Supported Work. In no instance during the post-enrollment period did welfare and food stamp bonuses constitute as much as 25 percent of total income and, by months 28 to 36, only about 15 percent of all income was unearned.[1]

B. RECEIPT OF IN-KIND ASSISTANCE

In addition to transfer income, 19 to 21 percent of the sample youth had Medicaid cards and 25 to 30 percent received some form of housing assistance during each of the 9-month follow-up periods.[2]

---

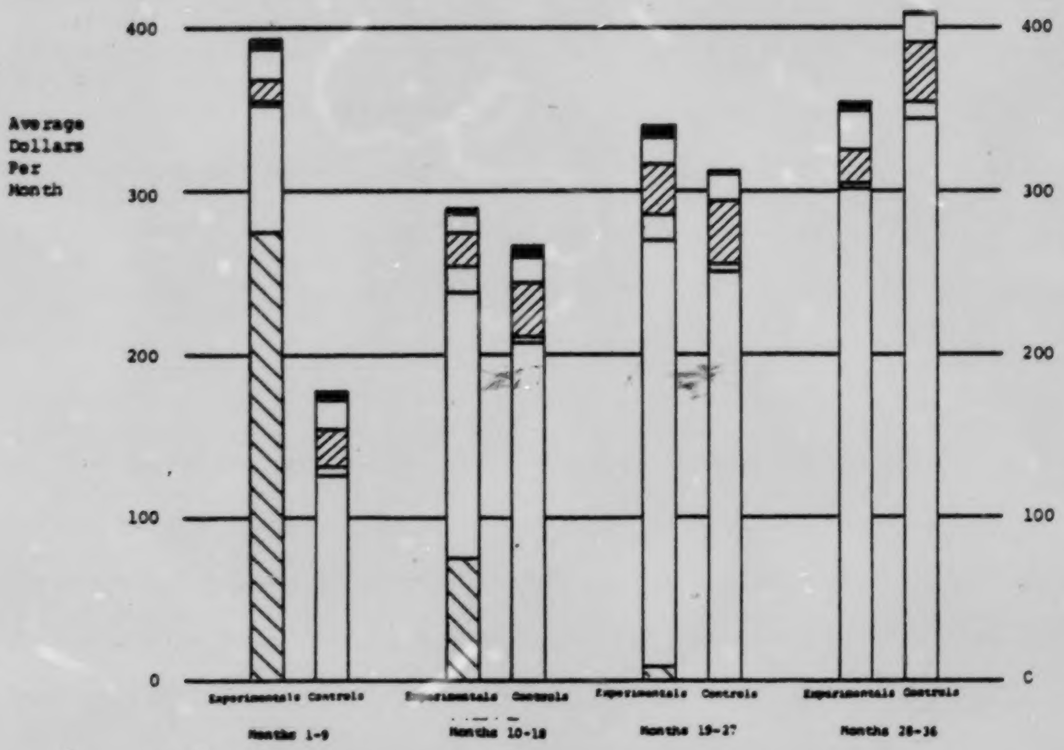
[1] There was some slight variance in dependence on transfer income across sites. Overall, youth in New York were less likely than average to receive benefits (and, consequently, program impacts on benefits were lower), and those in Hartford were slightly more likely than average to receive benefits (and, consequently, program impacts on benefit levels were larger than average). Perhaps because welfare and food stamps constituted such a small percentage of total income, we observed no relationship between the sites where welfare and where employment impacts tended to be relatively more favorable.

[2] Data on in-kind assistance are presented in Appendix Table A.24.

FIGURE IV.2

COMPONENTS OF TOTAL MONTHLY INCOME

YOUTH SAMPLE



NOTE: Data from which this figure was drawn are presented in Table IV.1b.

- ■ Other Unearned Sources
- ☐ Food Stamp Bonus
- ▨ Welfare
- ☐ Unemployment Compensation
- ☐ Non-Supported Work Earnings
- ▧ Supported Work Earnings

While program-induced income changes might be expected to alter eligibility for both types of benefits, the only significant findings with respect to Medicaid benefits were 6- and 9-point reductions in the percentage of experimentals relative to controls who had a Medicaid card during the first and second 9-month periods after enrollment, respectively. There is some indication that card holdership remained lower among experimentals than controls in later periods as well, but the estimated experimental-control differentials were not statistically significant. There is, however, no indication that this loss in benefits led to any reduction in public expenditures for subsidized care, as both experimentals and controls reported roughly equal numbers of subsidized doctor visits and subsidized days of hospital care.[1]

Throughout the period of observation, a smaller percentage of experimentals than controls lived in public housing, and those experimentals in public housing tended to pay slightly higher rents than did their control group counterparts. However, the experimental-control differentials were generally small (5 to 7 percentage points, or $2 to $19) and statistically significant only during the first 9-month period following enrollment.

C. OTHER RELATED OUTCOME MEASURES

To the extent that Supported Work is successful in its primary goal of enabling school dropouts, many of whom have some record of criminal activity, to become active members of the workforce and to increase their economic status, we might also expect to see changes in areas such as household composition, expenditures for housing, and medical-care utilization of participants and former participants. Other social programs, such as the negative income tax (NIT) experiments and Job Corps, have shown some evidence of impacts in these areas.[1]

---

[1] Furthermore, the loss of medical-assistance benefits did not result in differential consumption of medical care between experimentals and controls.

With respect to housing consumption, the one consistent finding is that a higher percentage of experimentals than controls were renting nonsubsidized units (61 versus 54 to 57 percent). However, these differences were statistically significant only for the first 9-month period. Experimentals and controls living in these nonsubsidized dwellings paid similar rents of about $145 per month. As noted previously, there was some tendency for fewer experimentals than controls to live in public housing, but the differentials were not large (5 to 7 percentage points) and were statistically significant only during the first 9 months following enrollment. During the 19- to 36-month period, experimentals lived in significantly larger dwelling units than did controls, but the number of rooms per person in the household was similar for experimentals and controls in all time periods.[2] Residential mobility, the incidence of home improvements, and crime victimization were similar between experimentals and controls throughout the period of observation. Similarly, Supported Work does not seem to have had much effect on the household composition of the youth sample. Household size ranged from four to five persons,[1] including an average of only 0.2 to 0.4 dependents.[2] Between 5 and 9 percent of the sample members reported being married in any 9-month period, but again there was no significant differential between experimentals and controls.

---

[1] Knudsen et al. (1977) and Hannan (1977) discuss marital-stability findings; Wooldridge (1977), Avrin (1978), and Ohls (1979) discuss housing impacts; and Kerachsky (1977) discusses health-care consumption results from the NIT experiments. Abt Associates (1979) discuss the related impacts of the Job Corps program.

[2] On average, this sample lived in dwellings with an average of 1.3 to 1.5 rooms per person, which is well above national standards for overcrowding (Heilbrun, 1973).

Finally, Supported Work had no significant effect on the use of health care by this sample. The sample averaged roughly one doctor visit and one day in the hospital in each 9-month period.[3] There seemed to be no relationship between health-care utilization and eligibility for Medicaid. Among the subsample of workers, both experimentals and controls lost an average of one day's work per month due to illness over the entire 36 months.

D. CONCLUSION

A Supported Work program for youth will have short-term benefits to participants in terms of substantially higher standards of living while they are in the program. For example, among those youth not living with

---

[1] During the last 9-month period (months 28 to 36) the average experimental household size was five, while the control average was four, a statistically significant difference.

[2] Over time, household size tended to decrease, largely as a result of youth moving out of their parents' homes: while about 70 percent lived with their parents at enrollment, only 56 percent did so two years later.

[3] Roughly 30 percent of the sample reported having seen a doctor and 10 percent reported having been hospitalized during each 9-month period. There was very little in the way of consistent experimental-control patterns in the reasons for seeking health care, except that in the first 18-month period significantly more experimentals than controls said they had visited the doctor as a consequence of job-related injuries.

their parents, the percentage with incomes below the poverty level was 35 points lower among the experimentals than the controls. Such changes in income are accompanied by some small benefits to taxpayers in the form of reduced transfer payments. However, after youth have left Supported Work, the impacts on both personal incomes and public subsidies will be small, at best. Over the first two years following enrollment in Supported Work, the net income gain per participant was almost $2,300, about $1,900 of which they received during the first 9 months. The net reduction in public-assistance benefits (welfare and food stamps) totaled less than $300,[1] while unemployment compensation benefits (mainly from the Special Unemployment Assistance program) increased by about $130. Impacts on other forms of transfers were similarly small. We are left with little reason to expect a Supported Work program for youth to affect substantially either the overall economic welfare of this segment of the population or its demands on our public-assistance programs. Furthermore, there is little evidence of any significant changes in the various other outcomes considered, such as household composition, housing quality, and medical-care utilization.

---

[1] These estimated effects are 4 to 5 percent smaller if expressed in constant third-quarter 1976 dollars.

CHAPTER V

IMPACTS ON DRUG USE

The national increase in drug use which began in the 1960s continued into the mid-1970s, particularly among youth (DuPont, 1978). It was estimated that, in 1977, 70 percent of all those age 18-25 were using alcohol, 30 percent marijuana, and almost 4 percent drugs such as heroin, other opiates, cocaine, amphetamines, and stimulants (Abelson et al., 1977). Further, drug use has generally been found to be higher among youth in urban areas, those who are unemployed, and those who have lower levels of education (O'Donnell et al., 1976)--characteristics that also describe the Supported Work youth sample. The concern over these rising trends stems, in part, from recognition that drug use may exacerbate youth's employment problems and that it may lead to increased participation in crime (O'Donnell et al., 1976; Trice and Roman, 1972; Voss, 1976; Jessor, 1976). However, there are also other causes for this concern, including the impact of drug use on health status and productivity, and the social costs incurred for treatment and prevention.

As noted in Chapter II, both sociological and economic theories of the causes of drug abuse suggest that Supported Work may affect the prevalence of drug use. But the direction of the effect is not clear, particularly in view of the fact that, contrary to expectations, the youth in the Supported Work sample did not have an unusually high prevalence of drug use prior to their enrollment in Supported Work (see Table V.1).[1] The one exception is their somewhat higher than average use of heroin.

---

[1] All three sets of data on lifetime use are based on personal interview responses. Thus, there is little reason to expect that there was differential misreporting.

TABLE V.1

LIFETIME DRUG USE OF YOUTH ENROLLED IN SUPPORTED WORK VERSUS NATIONAL SAMPLES

	Percentage Reporting Ever Having Used Drug		
	Supported Work Sample[a]	National Sample of 20-Year-Olds[b]	National Sample of 18- to 25-Year-Olds[c]
Alcohol	72	95	84
Marijuana	61	59	60
Cocaine	14	16	19
Heroin	8	4	4
Other Opiates	2	33	14
Amphetamines	7	26	21
Barbiturates	8	23	18
Psychedelics	10	30	20

[a] These data were obtained from enrollment interviews and apply to all youth in the Supported Work research sample (Jackson et al., 1978).

[b] These data are based on in-person interviews with a sample of 290 20-year-olds, conducted in 1974 and 1975 as part of a study of non-medical use of psychoactive drugs by young men in the United States. The overall sample for the study was a multi-staged stratified random sample (see O'Donnell et al., 1976).

[c] These data are based on interviews with a national sample of 1,500 18- to 25-year-olds, conducted in 1977 as part of a nationwide survey of persons age 12 and older living in households in the contiguous United States (see Abelson et al., 1977).

Below, we examine the evidence concerning Supported Work's effect on drug use in the youth group. Since use of alcohol and marijuana is particularly prevalent among youth in this sample and since heroin and cocaine use are thought to have serious social consequences,[1] these are the four drugs we have chosen to focus on.[2] In addition, we have considered two summary measures of drug use: a measure of whether the individual used any drug other than marijuana or alcohol, and an index that weights the use of various drugs according to an estimate of the marginal impact of the use of each on the number of arrests one is likely to incur.[3] In all instances, the outcome measures are based on self-reports of any use (or daily use) of the drug during the previous nine months.[4]

A. OVERALL PROGRAM IMPACTS

As shown by the results presented in Table V.2, Supported Work has had no overall effect on the prevalence of drug use. During each

---

[1] Analyses based on the ex-addict and ex-offender Supported Work samples indicate a strong positive correlation between both heroin and cocaine use and the number of times one is arrested (Dickinson, 1980).

[2] Less than 1 percent of the youth reported having used opiates other than heroin, or amphetamines, barbiturates, or psychedelics during each of the 9-month follow-up periods.

[3] Dickinson (1980) describes the development of this drug-use index.

[4] The quality of self-reported data on drug use is, of course, questionable. While there is evidence that such reports will understate the use rates (O'Donnell et al., 1976), there is little reason to expect differential underreporting by experimentals and controls. Equal proportional underreporting by experimentals and controls still poses two potential analytic problems, however. One is that the absolute value of any program effect will be biased toward zero, and the other is that tests of the statistical significance of estimated differentials will tend to be conservative.

TABLE V.2

PERCENTAGE REPORTING USE OF DRUGS, BY TYPE OF DRUG

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Any Drug (other than marijuana or alcohol)	-2.9	14.2	0.3	10.2	0.4	10.6	5.8	11.0
Heroin								
Any use	0.4	3.6	-0.7	2.4	0.6	1.2	0.9	1.0
Cocaine								
Any use	-1.1	8.2	-1.2	8.2	-1.0	8.4	5.7	9.7
Marijuana								
Any use	4.5	52.4	1.6	51.2	0.3	57.6	0.1	64.1
Daily use	n.a.	n.a.	0.0	22.4	5.4	21.1	-0.3	29.4
Alcohol								
Daily use	2.6*	5.5	1.9	9.3	0.7	9.9	-1.6	8.9
(Index of Drug Use[a])	(-0.5)	(9.2)	(-1.3)	(8.9)	(0.0)	(6.3)	(2.9)	(7.1)

NOTE: See note to Table III.3.
Daily use of heroin, cocaine, and any use of other opiates, amphetamines, barbiturates, and psychedelics, was reported by less than 1 percent of the youth sample and so data for these categories are not included in this table.

[a] This index weights the use of each drug by its association with arrests. See Dickinson (1980) for a description of the methodology used to develop the index and for the actual weights used.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

n.a. = not available

9-month follow-up period, 10 to 17 percent of the youth used some drug other than marijuana; 1 to 4 percent used heroin; 7 to 15 percent used cocaine; 51 to 64 percent used marijuana; and 6 to 11 percent used alcohol on a daily basis. The only statistically significant difference in reported use between experimentals and controls was estimated for daily alcohol use during the first nine months after enrollment, when 8 percent of the experimentals compared with 6 percent of controls reported such use.[1] In subsequent periods, however, the percentages for the two groups were about equal (varying between 7 and 11 percent).

While little overall program impact on drug use was observed, it was possible that Supported Work had altered the relationship between drug use and employment. However, as seen in Table V.3, experimental-control differentials in drug use generally were not significantly different from zero for either those who were employed or those who were not employed.[2] A sizable portion of experimentals and controls in both employment statuses (7 to 20 percent) reported having used drugs other than marijuana or alcohol.

B. DIFFERENTIAL IMPACTS AMONG SITES AND ACROSS SUBGROUPS OF YOUTH

Despite the lack of overall program impacts, it is possible that Supported Work programs at some of the sites did affect drug use significantly, either because of the nature of the program experiences themselves

---

[1] When estimated using probit analysis, this estimated differential, while about the same magnitude, was not statistically significant, however.

[2] The one exception is that a significantly lower percentage of experimentals than controls who were employed reported using drugs during months 1 to 9. However, this is related to very high use rates by employed controls relative to unemployed controls. Generally similar results to these for use of any drugs were also observed for alcohol use.

TABLE V.3

PERCENTAGE REPORTING USE OF ANY DRUG, OTHER THAN MARIJUANA OR ALCOHOL, BY CURRENT EMPLOYMENT STATUS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
Employment Status	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Not Employed	2.5	10.1	-2.0	11.3	-1.9	8.6	12.0	8.0
Employed	-6.9**	18.1	1.3	9.8	1.3	12.4	-3.5	16.0
(Percentage Employed)[a]	(45.0)**	(53.0)	(8.5)**	(59.9)	(1.0)	(60.8)	(7.5)	(66.2)

NOTE: For definitions of the samples used, see Table II.2. These data are not regression adjusted.

[a] These data may differ somewhat from those reported in Chapter III because of the slight differences in the samples used and because these data are not regression-adjusted.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

or because of the peer-group structures. In particular, a priori one might expect there to be an increase in drug use among youth in programs where the peer group includes ex-addicts and ex-offenders and a decrease among youth in programs enrolling only AFDC women in addition to youth. Similarly, one might expect relatively more favorable impacts among those sites with the most supportive supervisors.

Table V.4 presents estimates for each of the five sites of program impacts on the use of some drug other than marijuana or alcohol, use of marijuana, and daily use of alcohol, during each of the first three 9-month periods.[1] There are few significant experimental/control-group differences. In only one case where there is a significant difference is the sign of the estimated impact consistent over time; among the Hartford sample, a higher percentage of experimentals than controls reported using alcohol daily, and the 6 percentage-point differential in the first 9-month period is statistically significant. In four other cases the pattern of results across time is consistent, although none of the point estimates of the impacts is significantly different from zero. These cases are the estimated reduction in use of any drugs among the Jersey City experimentals, the reduction in marijuana use among Philadelphia experimentals, and the increases in the use of marijuana among experimentals relative to controls in the Hartford and Jersey City samples.[2] These results are not

---

[1] For two reasons, subgroup results were not estimated for the 28- to 36-month period. The first is that the sample size is very small, and the second is that the preenrollment drug-use experiences of the 36-month sample are not at all representative of the group as a whole, as evidenced by the data in Table II.3.

[2] Any use of drugs is relatively high among Jersey City controls and use of marijuana is relatively prevalent among Philadelphia controls as compared with controls in other sites. However, there is no consistent pattern of the control group means associated with the higher rates of marijuana use among experimentals.

TABLE V.4

PERCENTAGE REPORTING USE OF VARIOUS DRUGS BY SITE

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental Control Differential	Control Group Mean
A. Any Drug (other than Marijuana or Alcohol)						
All Youth	- 2.9	14.2	0.3	10.2	0.4	10.6
Site					#	
Atlanta	- 7.6	18.0	0.5	8.1	1.4[b]	6.3
Hartford	- 0.5	11.7	1.8	7.0	2.6	7.5
Jersey City	- 6.5	17.3	- 2.5	18.7	- 5.3	18.1
New York	1.4	14.6	- 6.5	15.6	0.9[b]	14.6
Philadephia	- 9.6	13.2	12.9*	- 4.1[a]	5.9	3.5
B. Marijuana						
All Youth	4.5	52.4	1.6	51.2	0.3	57.6
Site						
Atlanta	11.4	57.3	11.9	43.8	-42.9*[b]	82.1
Hartford	7.6	48.1	1.5	43.6	2.8	55.3
Jersey City	8.2	53.5	3.2	62.5	2.4	57.5
New York	- 4.1	54.9	- 4.1	63.9	24.7[b]	48.9
Philadephia	-20.7	70.2	- 4.7	54.2	- 7.3	61.7
C. Daily Use of Alcohol						
All Youth	2.6*	5.5	1.9	9.3	0.7	9.9
Site						
Atlanta	- 3.3	10.8	2.0	7.3	-15.2[b]	17.4
Hartford	5.7**	8.4	0.5	7.6	5.1	4.2
Jersey City	- 3.8[a]	0.3	2.9	15.4	4.6	13.0
New York	8.4**	3.5	2.4	10.4	-16.5[b]	21.0
Philadephia	- 0.6[a]	0.5	6.3	1.6	-14.3**	16.2

NOTE: See note to Table III.3.
Sample sizes for the various sites can be computed by multiplying the proportion of the sample in the site by the total sample size. (These figures are presented in Appendix Table A.4.) There were too few observations to permit meaningful disaggregation of the 28- to 36-month sample.

[a] Negative point estimates of experimental or control group means arise because, as discussed in Chapter II, linear regression analysis rather than probit analysis was used.

[b] Sample size is 20 or less.

#Experimental-control differentials vary significantly among the sites. (Section II.E describes the test procedures used.)

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

consistent with a priori notions of expected site differentials in program impacts--namely, that drug use might increase among experimentals relative to controls in sites enrolling ex-addicts and/or ex-offenders.

Program impacts on drug use might also be expected to vary among youth with different demographic and background characteristics, in which case knowledge of such impacts could be useful in deciding on program targeting strategies. Tables V.5, V.6, and V.7 display estimated program impacts, for a number of different subgroups of the youth sample, on use of any drug, use of marijuana, and daily use of alcohol.

Overall, no pattern identifies a particular group for whom Supported Work will reduce the prevalence of drug use. There are few significant experimental-control differences in use of any drugs other than marijuana or alcohol, and in only a few cases are the estimated differences for a subgroup both of consistent sign in the three periods and significant in at least one. In all three periods, it is estimated that white experimentals, those with some brief period of prior work, and those whose best friend does not use drugs are less likely than their control group counterparts to have used drugs. However, in most cases, the estimated differentials are extremely small (less than 1 percentage point).

For marijuana use, the estimates of impacts for the various subgroups tend to be reasonably consistent in sign across time and the magnitudes of the estimated impacts, furthermore, are often reasonably large. The pattern of results suggests that Supported Work may be more likely to lead to increased use of marijuana among those who, as compared to other groups of youth, are Spanish, have fewer than nine years of education, are not receiving welfare at enrollment, and live in neighborhoods with

TABLE V.5

PERCENTAGE REPORTING USE OF ANY DRUG, BY DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth	-2.9	14.2	0.3	10.2	0.4	10.6
Years of Age						
Under 19	-1.6	13.5	-1.8	11.2	-2.1	13.0
19 or older	-4.5	15.1	3.7	8.7	3.5	7.2
Sex			#		#	
Male	-1.7	13.9	-1.0	11.4	2.8	9.5
Female	-9.5	15.6	9.7*	2.8	-21.4**	19.4
Race/Ethnicity					#	
White, not Hispanic	-0.5	19.7	-7.8	15.1	-20.6** a/	27.2
Black, not Hispanic	-3.4	14.4	0.7	9.9	0.5	9.4
Hispanic	-0.5	10.7	2.3	9.8	11.7	6.7
Years of Education						
8 or less	-2.3	9.9	4.8	5.6	0.1	6.9
9 or more	-2.8	14.9	-0.3	11.0	0.2	11.3
Welfare and Food Stamp Receipt in Month Prior to Enrollment b/						
None	-1.9	15.1	0.8	10.2	-0.9	10.7
Some	-4.4	12.3	-0.3	10.1	2.7	10.5
Dependents						
None	-3.2	13.9	0.8	10.0	-0.2	10.9
One or more	1.1	16.6	-3.0	12.1	4.8	7.8
Months in Longest Job						
0	1.8	14.6	3.0	7.4	2.2	8.4
1 - 12	-5.4**	14.6	-0.4	11.0	-0.5	12.2
More than 12	7.7	9.2	0.6	11.5	0.9	3.9

Table V.5 (continued)

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Weeks Worked In Year Prior to Enrollment[c]						
0	0.3	10.3	-0.3	10.9	-0.8	9.9
5	-1.3	12.3	0.1	10.5	-0.3	10.5
10	-2.9	14.3	0.5	10.1	0.2	11.0
Prior Drug Use						
Used drugs other than marijuana	-3.1	32.4	-4.9	22.8	-5.4	24.1
Did not use any drug other than marijuana	-2.6	8.7	2.0	6.7	2.6	4.9
Addicts in Neighborhood[d]						
Few or none	-0.5	11.6	-0.8	9.7	0.6	8.2
Many	-8.3**	19.0	1.9	10.7	-0.7	15.7
Best Friend[d]	#					
Does not use drugs and is not involved in crime	-5.6**	14.1	-1.0	9.0	-0.6	10.5
Uses drugs or is involved in crime	8.5*	13.5	4.7	14.3	3.1	11.1
Prior Arrests[c]						
0	-4.2	14.6	-0.7	10.8	4.2	8.7
4	-1.6	13.7	1.3	9.6	-2.2	12.6
9	1.4	15.7	4.9	11.6	-1.4	10.6
Months Since Incarceration						
Never incarcerated	-0.7	11.8	-2.5	9.3	-1.4	11.1
12 or less	-4.3	21.7	7.4	13.4	3.0	11.6
More than 12	-13.4*	17.2	8.6	10.9	2.8	8.2

NOTE: See note to Table V.4. There were too few observations in the 28- to 36-month sample to permit disaggregation into subgroups.

[a] Negative point estimates of experimental or control group means arise because, as discussed in Chapter II, linear regression analysis rather than probit analysis was used.

[b] Welfare includes AFDC, General Assistance, and other welfare or welfare income for which respondent could not identify the source.

[c] These estimates of subgroup effects and means are based on a linear specification of the sample characteristic, evaluated at the specified points.

[d] These results were obtained from a regression that did not include the full set of variables interacting status with background characteristics.

[#] Experimental-control differentials vary significantly among the subgroups. (Section II.E describes the test procedure used.)

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE V.6

PERCENTAGE REPORTING USE OF MARIJUANA, DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth	4.5	52.4	1.6	51.2	0.3	57.6
Years of Age						
Under 19	6.0	51.8	0.3	53.2	4.8	59.9
19 or older	2.9	53.3	4.4	48.2	-6.1	54.4
Sex					#	
Male	6.0	52.9	3.6	52.0	3.2	58.4
Female	-2.4	49.7	-7.3	46.3	-24.2*	51.4
Race/Ethnicity					#	
White, not Hispanic	-8.8	62.2	-22.3	74.5	-33.5**	71.0
Black, not Hispanic	2.9	56.4	1.3	52.7	-3.1	61.2
Hispanic	17.4**	31.3	13.1	37.0	39.0**	30.3
Years of Education						
8 or less	16.4*	34.3	10.2	38.9	15.4	46.2
9 or more	2.8	55.4	0.6	53.3	-2.7	59.9
Welfare and Food Stamp Receipt in Month Prior to Enrollment [a/]						
None	9.1*	50.4	3.2	52.0	5.2	55.3
Some	-3.2	56.0	-0.3	49.7	-10.1	62.3
Dependents						
None	5.8	52.1	3.9	50.8	2.0	56.6
One or more	-4.4	54.8	-13.6	54.8	-17.9	68.2
Months in Longest Job						
0	-5.9	56.8	-6.1	52.8	-7.7	59.1
1 - 12	7.1*	53.0	4.5	52.5	-0.2	59.4
More than 12	18.7	30.6	5.9	32.7	29.2	38.1

Table V.6 (continued)

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Weeks Worked In Year Prior to Enrollment[b]						
0	6.9	52.8	6.9	48.5	2.4	58.6
5	5.7	56.6	4.2	50.0	1.4	58.1
10	4.5	52.3	1.4	51.5	0.3	57.6
Prior Drug Use						
Used drugs other than marijuana	11.5	65.8	2.6	60.4	3.9	62.0
Did not use any drug other than marijuana	3.0	49.0	1.8	48.9	-1.2	55.6
Addicts in Neighborhood[c]					#	
Few or none	3.9	52.0	-2.9	54.0	-7.9	59.9
Many	5.6	53.3	10.5	45.5	15.5**	53.5
Best Friend[c]					#	
Does not use drugs and is not involved in crime	2.1	53.0	3.3	48.1	4.9	54.7
Uses drugs or is involved in crime	16.0*	49.8	-7.4	66.3	-13.7	66.6
Prior Arrests[b]			#			
0	5.9	48.5	8.4	43.5	13.4	56.4
4	3.9	59.0	-3.2	58.2	-7.1	58.2
9	8.0	60.7	7.6	59.7	-8.8	59.9
Months Since Incarceration					#	
Never incarcerated	5.0	51.6	4.7	49.5	-9.4	59.6
12 or less	4.9	51.3	-7.2	59.8	15.7	52.0
More than 12	2.6	60.1	-3.4	50.7	16.1	56.5

NOTE: See note to Table V.4.
There were too few observations in the 28- to 36-month sample to permit disaggregation into subgroups.

[a] Welfare includes AFDC, General Assistance, and other welfare or welfare income for which respondents could not identify the source.

[b] These estimates of subgroup effects and means are based on a linear specification of the sample characteristic, evaluated at the specified points.

[c] These results were obtained from a regression that did not include the full set of variables interacting status with background characteristics.

#Experimental-control differentials vary significantly among the subgroups. (Section II.E describes the test procedure used.)

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE V.7

PERCENTAGE REPORTING DAILY USE OF ALCOHOL, BY DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth	2.6*	5.5	1.9	9.3	0.7	9.9
Years of Age			#		#	
Under 19	3.7*	5.4	2.7	9.3	-2.9	11.9
19 or older	1.5	5.7	0.4	9.2	6.3	7.0
Sex						
Male	2.6	6.3	1.6	10.8	0.3	10.7
Female	3.7	0.3	2.7	0.0	5.1	2.9
Race/Ethnicity						
White, not Hispanic	6.7	1.8	6.5	10.0	-7.8 a/	6.7
Black, not Hispanic	2.9	6.3	2.9	9.7	0.9	11.6
Hispanic	0.8	1.6	-5.4	7.1	5.9	2.8
Years of Education			#			
8 or less	1.6	5.9	11.9**	3.4	10.1	3.6
9 or more	3.0*	5.4	0.0	10.3	-1.0	11.1
Welfare and Food Stamp Receipt in Month Prior to Enrollment b/						
None	5.0**	4.5	3.5	6.8	0.4	8.2
Some	-1.4	7.3	-1.4	13.7	1.7	13.7
Dependents						
None	2.5	5.6	1.7	9.6	1.7	9.7
One or more	5.1	5.0	2.9	6.4	-7.9	11.9
Months in Longest Job						
0	3.4	5.8	3.5	9.0	-4.5	13.8
1 - 12	3.2*	4.9	1.5	9.1	3.4	8.5
More than 12	-2.9	10.0	-1.0	11.6	-5.5	10.0

Table V.7 (continued)

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Weeks Worked In Year Prior to Enrollment [c]						
0	4.2*	4.6	1.8	10.2	0.1	8.1
5	3.4*	5.0	1.8	9.7	0.4	8.5
10	2.7*	4.6	1.8	9.2	0.8	8.9
Prior Drug Use						
Used drugs other than marijuana	1.2	6.5	-1.8	16.9	2.5	15.0
Did not use any drug other than marijuana	3.2*	5.2	2.9	7.0	0.1	7.7
Addicts in Neighborhood [d]						
Few or none	2.7	5.1	0.8	10.1	-3.6	10.5
Many	2.1	6.4	4.5	6.8	8.7*	8.7
Best Friend [d]						
Does not use drugs and is not involved in crime	3.4*	4.5	2.4	8.6	-0.5	8.7
Uses drugs or is involved in crime	-1.0	9.4	0.0	12.3	4.2	13.4
Prior Arrests [c]					#	
0	1.7	4.7	0.2	9.5	2.9	5.1
4	3.6	6.1	3.2	9.1	-1.3	11.5
9	5.9	6.1	1.2	8.8	8.4*	7.5
Months Since Incarceration	#					
Never incarcerated	2.1	5.8	1.6	8.8	-0.3	9.9
12 or less	9.8**	4.4	6.7	7.5	3.0	13.6
More than 12	-3.4	5.7	-4.1	15.3	2.2	6.3

NOTE: See note to Table V.4.
There were too few observations in the 28- to 36-month sample to permit disaggregation into subgroups.

[a] Negative point estimates of experimental or control group means arise because, as discussed in Chapter II, linear regression analysis rather than probit analysis was used.

[b] Welfare includes AFDC, General Assistance, and other welfare or welfare income for which respondents could not identify the source.

[c] These estimates of subgroup effects and means are based on a linear specification of the sample characteristic, evaluated at the specified points.

[d] These results were obtained from a regression that did not include the full set of variables interacting status with background characteristics.

#Experimental-control differentials vary significantly among the subgroups. (Section II.E describes the test procedure used.)

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

many addicts. There is also some indication that female and white participants may reduce their marijuana use more than their control group counterparts. However, as with the results for use of drugs in general, the weight of the evidence is that there is no significant impact for any subgroup.

The subgroup results for daily alcohol use are more mixed than those for marijuana use, but they also tend to suggest that if the program had any impact it was to increase slightly the prevalence of alcohol use among the groups with similar characteristics as those among whom marijuana use increased.

C. CONCLUSIONS

The conclusion one must draw from this analysis is that Supported Work had no significant effect on the drug use of youth participants. The evidence that overall null results may be due to offsetting positive results for some groups and negative impacts for others is weak, at best.

Finally, a comparison of drug use and alcohol use between experimentals and controls who were not employed and between experimentals and controls who were employed revealed no consistent pattern of differences. At least among youth similar to those enrolled in Supported Work, then, drug use seems to be independent of employment status, perhaps because, as discussed in Chapter II, the income effect of employment offsets its sociological effects.

CHAPTER VI

IMPACTS ON CRIMINAL BEHAVIOR

As noted in Chapter II, both sociological and economic theories of criminal behavior suggest that successful integration of youth into the labor force might be expected to reduce their likelihood of participating in criminal activities. In this chapter, we use a number of indicators of criminal behavior to investigate the extent and nature of any impacts of Supported Work on illegal activities of young school dropouts, about 40 percent of whom have previously been convicted of a criminal offense. The various indicators we discuss include self-reported data on arrests, convictions, and incarcerations.

Self-reports of crime commissions and income from illegal activities were collected;[1] however, they are of questionable quality. For this reason and because some previous validation work in conjunction with this demonstration and other studies has been undertaken to assess the quality of self-reported arrest data, we have opted to rely on reports of criminal justice experiences.[2]

---

[1] Between 3 and 17 percent of the sample youth reported engaging in illegal activities (mainly theft and selling drugs) during each 9-month period.

[2] For example, a comparison of self-reported arrest data with official records data for a sample of 774 ex-addicts and ex-offenders enrolled in the Supported Work demonstration has shown that individuals reported only 54 percent of the arrests they incurred, but that experimental and control group members underreported by a similar percentage (Schore et al., 1979). While these results may or may not be generalizable to the youth sample, the general implication is that estimates of program-induced changes in arrests are expected to be understated and, in the case of binomial outcome measures, the test of statistical significance will be conservative.

Piliavin and Gartner (1980) provide a more detailed justification for the outcome measures focused on in the Supported Work evaluation studies.

In some respects, the best measure of program impacts is the percentage arrested, as this is a clear indication of program failure, in that arrests are highly correlated with crime commission.[1] However, the others can provide useful information as to the seriousness and consequences of the offenses for which individuals were arrested. For example, we have included data on robbery arrests because of their high social costs[2] and because there is reason to expect Supported Work to have its greatest impact on economically motivated crimes such as robbery. There was also special interest in program effects on drug-related arrests, because of the hypothesis that Supported Work might reduce drug use and because such arrests are likely to stem from economic transactions. Although subject to distortion because of delays in the criminal justice system's processing of arrests, the convictions and incarceration data provide yet other indications of the criminal involvement by experimental and control youth.[3]

---

[1] Of course, arrests do not indicate guilt. Studies have related the incidence of arrests to that of crime commissions, thus providing a necessary link for the companion benefit-cost analysis.

[2] See Kemper et al. (1980) for a discussion of the social, participant, and nonparticipant costs and benefits associated with various types of arrests. Silberman (1978) discusses the great public concern with robbery.

[3] It is also possible that the Supported Work programs may intervene in the judicial process and thereby affect the disposition of experimentals' arrest charges.

A. OVERALL PROGRAM IMPACTS

On average, the Supported Work employment opportunity has not had a significant impact on criminal behavior among sample youth, either during the period when individuals were working in their program jobs or subsequently.

1. Results During 9-Month Periods Following Enrollment

As seen in Table VI.1, during each of the first two 9-month periods following enrollment in Supported Work, about 16 percent of both the experimentals and controls reported having been arrested and, among those arrested, the average number of arrests per sample member was between 1.2 and 1.5. Between 15 and 20 percent of the arrests were for robbery, and less than 10 percent were for drug-related offenses. A sizable portion of the arrests did lead to conviction and to incarceration. However, there again is no significant difference between experimentals and controls.

The results for months 19 to 27 show a somewhat more favorable pattern, in that a lower percentage of experimentals than controls reported having been arrested in months 19 to 27 (11 versus 14 percent), a lower percentage were convicted (4 versus 7 percent), and the experimentals spent an average of 30 percent less time in jail than did controls (2.6 versus 3.7 weeks). However, none of these effects is statistically significant.

While the results still are not statistically significant, the pattern for months 28 to 36 is generally not favorable. A higher percentage of experimentals than controls were arrested (23 versus 17 percent) and incarcerated (20 versus 17 percent), while slightly lower percentages

TABLE VI.1

ARRESTS, BY TYPE OF OFFENSE, CONVICTIONS, AND INCARCERATION

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Percentage with any Arrest	0.3	16.8	1.6	15.2	-3.2	13.6	6.4	16.7
Number of Arrests	0.06	0.20	0.03	0.18	-0.05	0.16	0.09	0.18
Percentage with Robbery Arrests[a]	-0.3	3.4	0.6	2.6	-1.0	3.1	2.2	2.3
Number of Robbery Arrests[a]	-0.00	0.04	0.00	0.03	-0.01	0.04	0.02	0.02
Percentage with Drug-related[b] Arrests	1.0	0.9	-0.5	1.2	0.8	0.4	1.6	1.3
Percentage Convicted	1.2	9.1	0.0	8.3	-2.3	6.7	-1.8	9.8
Percentage Incarcerated	-2.7	11.6	2.0	12.6	3.8	15.5	2.5	17.2
Number of Weeks Incarcerated	-0.58	1.62	-0.14	2.37	-1.06	3.66	-1.12	3.67

NOTE: See note to Table III.3. All data pertain to the full sample.

[a] Robbery arrests are defined as those for which robbery was the most serious charge. Only murder and felonious assault are considered to be more serious than robbery.

[b] Drug-related arrests are defined as those for which narcotics-law violation is the most serious charge. More serious charges include murder, felonious assault, robbery, burglary, larceny, motor-vehicle theft and other property crimes, and other crimes against persons.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

were convicted (8 versus 10 percent).[1]

When we looked at experimental-control differentials separately for those employed and not employed, we observed no consistent pattern of effects. For example, during months 1 to 9, reductions in the percentage arrested were observed only among those experimentals not employed. During the 10- to 18-month period, reductions were not observed for experimentals in either subgroup, but in months 19 to 27, significant reductions were observed for employed experimentals, while increases were observed among those experimentals who were not employed.[2] In particular, the lack of any relationship between employment and arrest rates during the first 9 months when experimental group members participated in Supported Work suggests that for this sample, the lack of employment opportunities may not be a principal factor in criminal behavior.

2. Cumulative Results During the 18 and 27 Months Following Enrollment[3]

Cumulative measures of criminal activities over the follow-up period provide a slightly different view of program impacts. In particular, small impacts during the 9-month intervals might compound to result in significant long-run effects.

---

[1] Recall that the sample size for these later period results is sufficiently small that sampling error in estimates of program effects is large. For example, the 6 percentage-point difference in arrest rates is due to a difference of only four arrests for the total sample (16 versus 12).

[2] See Appendix Table A.25.

[3] Only 79 persons had continuous data for the 36 months following enrollment. Thus, results for months 1 to 36 were not estimated.

As seen in Table VI.2, results for the first 18 months following enrollment indicate that Supported Work had no effect on criminal activities, which is consistent with the findings for each of the two 9-month periods (Table VI.1). However, over the full 27-month period following enrollment, there is evidence of positive program impacts, suggesting that small differences had been accumulating over the three 9-month periods. During this full period, only 30 percent of the experimentals, as compared with 39 percent of the controls, reported having been arrested since enrollment in the demonstration sample, and a large share of this reduction (35 percent) is due to a reduction in experimentals' arrests for robbery. While not a statistically significant difference, a lower percentage of experimentals than controls were convicted during this period (20 versus 24 percent), and a significantly lower percentage of experimentals than controls were incarcerated (18 versus 28 percent).

3. Impacts for Subgroups with Varying Amounts of Follow-Up Data

One of the first questions that arises when comparing the results in Tables VI.1 and VI.2 is whether the favorable results for the 1- to 27-month period are due in part to differences in program response among those with varying amounts of follow-up data. In order to address this issue, results for the probability of an arrest were estimated for subgroups of the samples defined by the length of the interview follow-up period.

TABLE VI.2

CUMULATIVE ARRESTS, CONVICTIONS, AND INCARCERATION

YOUTH SAMPLE

	Months 1 - 18		Months 1 - 27[a]	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Percentage with any Arrest	-0.3	27.0	-8.8*	39.3
Number of Arrests	0.07	0.38	0.01	0.62
Percentage with Robbery Arrests[b]	0.4	6.1	-3.1	13.0
Number of Robbery Arrests[c]	0.00	0.07	-0.03	0.15
Percentage with Drug-related Arrests[c]	-0.1	2.1	0.9	4.9
Percentage Convicted	0.5	16.0	-4.0	23.6
Percentage Incarcerated	-0.6	18.3	-10.2**	28.0
Number of Weeks Incarcerated	-0.7	4.1	-4.8**	10.2

NOTE: See Note to Table III.3. Results for the 1- to 36-month period are not presented because of the limited sample size (79).

[a] The sample for this period includes people who completed baseline, 9-month, 18-month, and 27-month interviews. Therefore, the experimental and control-group values implied by these data are not consistent with those reported for the individual 9-month periods (see Table VI.1).

[b] Robbery arrests are defined as those for which robbery was the most serious charge. Only murder and felonious assault are considered to be more serious than robbery.

[c] Drug-related arrests are defined as those for which narcotics-law violation is the most serious charge. More serious charges include murder, felonious assault, robbery, burglary, larceny, motor-vehicle theft and other property crimes, and other crimes against persons.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

As seen from Table VI.3, among those with only 18 months of follow-up data, a higher percentage of experimentals than controls were arrested in both 9-month periods, as well as over the full 18-month period.[1/] However, in most time periods, results for those with 27 or 36 months of follow-up data indicate reductions in arrest rates among experimentals relative to controls that accumulate over time to result in differences of 8 to 11 percentage points over the 1- to 27-month period. Lack of statistical significance of these 8 and 11 percentage-point differences appears to be due to small sample sizes, since the overall estimate of the program effect is significant (Table VI.2). Unfortunately, data for the 1- to 36-month period are available for too few sample members (79) to permit calculation of reliable estimates of cumulative effects over this period. But evidence from the 28- to 36-month data suggest that the favorable pattern of cumulative results for the 27- and 36-month subsamples may not persist in later time periods.

B. DIFFERENTIAL IMPACTS ACROSS SITES

Impacts might be expected to vary across sites because of differences in criminal histories of sample members, program characteristics, or local labor-market conditions, as well as various unmeasured differences in characteristics of program enrollees. As seen in Table VI.4, experimental-control differences are consistently positive for the New York sample, and most often positive for the Jersey City and Philadelphia samples.[2/]

[1/] It is unlikely, but possible, that cumulative results would differ in sign from those for the individual periods.

[2/] Tobit estimates of the number of arrests incurred during the various time periods (see Appendix Table A.26) yield results that are similar to those reported in Table VI.4.

TABLE VI.3

PERCENTAGE ARRESTED BY LATEST FOLLOW-UP INTERVIEW

A. Nine-Month Period Results

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
18 months of follow-up	3.2	13.4	3.0	12.4	n.a.	n.a.	n.a.	n.a.
27 months of follow-up	-2.5	22.6	4.7	16.9	-3.7	16.0	n.a.	n.a.
36 months of follow-up	-4.1	13.8	-12.0*	25.8	-5.4	7.4	6.4	16.7

B. Cumulative Results

	Months 1 - 18		Months 1 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
18 months of follow-up	3.8	22.2	n.a.	n.a.
27 months of follow-up	-1.9	32.4	-0.1	41.4
36 months of follow-up	-13.4	32.4	-10.9	32.4

NOTE: Together, these subsamples include the same individuals as were included in the sample used to generate the data presented in Table VI.1. This total sample has been partitioned according to the most recent scheduled interview completed. Sample members used for the 19- to 27- and 28- to 36-month outcomes need not have completed all previously scheduled interviews, while those used to estimate the results over the 27-month period, of course, completed all previously scheduled interviews.

Among those with data for months 1 to 18, 36 percent completed a 27-month interview and 11 percent completed a 36-month interview; among those with data for months 19 to 27, 28 percent completed a 36-month interview; and among those with data for the 1- to 27-month period, 23 percent completed a 36-month interview.

*Statistically significant at the 10 percent level.

n.a. means not available.

TABLE VI.4

PERCENTAGE ARRESTED, BY SITE

YOUTH SAMPLE

	Months 10 - 18		Months 1 - 18		Months 19 - 27		Months 1 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth	1.6	15.2	-0.3	27.0	-3.2	14.0	-8.8*	39.3
Site								
Atlanta	-1.4	9.9	0.0	15.0	1.5[a]	1.6	-28.5[a]	33.5
Hartford	1.0	7.0	1.0	21.3	-6.6	19.0	-6.8	43.4
Jersey City	3.2	12.2	-4.6	25.8	-4.1	12.0	-10.7	35.8
New York	7.3	25.9	3.4	33.0	29.0*[a]	-4.3[b]	15.9[a]	21.7
Philadelphia	-10.5	21.9	-7.9	31.5	-1.8	12.8	-15.2	42.7

NOTE: See note to Table III.7.

[a] These data are based on a sample of fewer than 20 persons.

[b] This negative estimate for the control-group mean arose because linear regression as opposed to probit analysis was used (see Chapter II).

*Statistically significant at the 10 percent level.

Thus, much of the differential impact among subgroups with varying amounts of follow-up data is related to the site composition of the sample: nearly half of those with 27 or more months of data are from Jersey City and Philadelphia and less than 4 percent are from New York, while only 11 percent of those with 18 months of data are from Jersey City and Philadelphia, but 27 percent are from New York. Since the pattern of impacts on arrests is different from the employment and total income results (estimates of program impacts on employment and income are positive for Philadelphia and negative for Jersey City), it appears that employment and improved economic status per se were not the mechanisms through which these favorable impacts among selected sites occurred.

C. DIFFERENTIAL IMPACTS ACROSS SUBGROUPS OF YOUTH

There is little evidence to suggest that program impacts on the incidence of criminal activities, as indicated by the percentages arrested in each 9-month follow-up period, vary systematically across subroups of youth with various demographic and background characteristics (see Table VI.5). However, the program impacts on the percentages with any arrest over longer periods of time (i.e., any initiation of or recidivism to a delinquent or criminal life-style) suggest that the program is most effective in reducing involvement in crime among those who, in the absence of some form of intervention, exhibit a greater tendency toward such behavior: those who are younger, who are male, who have nine or more years of education, and who have previously worked in some job but who have little recent employment experience (see Table VI.6).[1]

---

[1] Subgroup results are not presented for months 28 to 36 and 1 to 36, since the expected number of persons arrested among those samples is about 25 in each. (There are 146 persons with valid data for the 28- to 36-month period and 79 with data for the full 1- to 36-month-period.)

TABLE VI.5

PERCENTAGE WITH ANY ARREST, BY DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth	0.3	16.8	1.6	15.2	-3.2	14.0
Years of Age						
Under 19	-2.4	19.8	0.6	15.2	-4.8	16.8
19 or older	4.7	12.6	3.7	15.3	-1.1	9.3
Sex						
Male	-0.2	18.7	1.6	13.8	-3.7	15.0
Female	4.7	5.8	3.5	23.8	-0.1	2.2
Race/Ethnicity						
White, not Hispanic	8.9	7.2	-11.2	21.3	-12.3	23.6
Black, not Hispanic	-0.7	18.3	2.6	15.4	-0.4	12.0
Hispanic	3.2	13.4	2.7	12.5	-13.2	15.8
Years of Education	#		#			
8 or less	11.5	9.8	19.3**	6.8	1.1	10.2
9 or more	-1.4	18.0	-1.1	16.7	-4.2	18.1
Welfare and Food Stamp Receipt in Month Prior to Enrollment a/			#		#	
None	-2.0	19.7	7.1**	13.5	0.9	12.4
Some	5.7	11.0	-8.8*	18.9	-13.7**	16.7
Dependents						
None	0.2	16.5	0.9	15.7	-4.7	13.8
One or more	3.0	20.0	9.3	11.8	11.4	12.1
Months in Longest Job						
0	9.6*	16.1	9.8*	14.5	-1.1	16.7
1 - 12	-1.6	17.0	-1.6	16.6	-3.7	12.8
More than 12	-9.2	17.7	8.5	5.9	-6.5	11.5

Table VI.5 (Continued)

	Months 1 - 9		Months 10 - 18		Months 19 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Weeks Worked In Year Prior to Enrollment[b]						
0	0.8	14.2	-1.0	16.5	-4.3	15.4
5	0.6	15.7	0.6	15.8	-3.8	14.6
10	0.5	17.2	2.2	15.1	-3.3	13.7
Prior Drug Use						
Used drugs other than marijuana	-4.5	22.2	4.4	17.4	-5.5	15.4
Did not use any drug other than marijuana	1.9	15.4	1.2	14.7	-2.4	13.0
Prior Arrests[b]						
0	-0.2	15.1	0.0	13.9	-1.1	11.4
4	1.2	18.5	3.6	16.5	-4.1	14.6
9	3.0	22.8	8.1	19.7	7.9	18.7
Months Since Incarceration	#		#			
Never incarcerated	-4.5	18.1	0.1	13.8	-2.2	13.7
12 or less	21.6	11.8	1.0	21.0	-11.8	20.3
More than 12	17.1	12.4	23.9	20.6	1.8	5.0
Parole or Enrollment						
Not on parole or probation	2.2	14.9	0.5	15.1	-6.4*	15.1
On parole or probation	-4.2	22.4	5.8	15.6	3.7	10.6

NOTE: See note to Table III.7. Results for the 28- to 36-month period are not presented because of the limited sample size (146) and the small number of sample members arrested during the period (29).

[a] Welfare includes AFDC, General Assistance, and other welfare or welfare income for which respondents could not identify the source.

[b] These estimates of subgroup effects and means are based on a linear specification of the sample characteristic, evaluated at the specified points.

#Experimental-control differentials within this subgrouping for this time period differ significantly from one another. (This test procedure is discussed in Section II.E.)

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE VI.6

CUMULATIVE PERCENTAGE WITH ANY ARREST, BY DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS

YOUTH SAMPLE

	Months 1 - 18		Months 1 - 27[a/]	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth	-0.3	27.0	-8.8*	39.3
Years of	#		#	
Under 19	-4.9	30.5	-16.4**	44.4
19 or older	6.9	21.9	2.2	29.6
Sex				
Male	-0.9	29.6	-10.5**	41.4
Female	4.9	11.5	8.2	13.6
Race/Ethnicity				
White, not Hispanic	0.4	23.6	6.8	27.3
Black, not Hispanic	-0.8	28.0	-9.6	39.2
Hispanic	3.4	23.1	-7.9	36.1
Years of Education	#		#	
8 or less	15.2*	14.5	20.2	20.6
9 or more	-2.6	28.8	-13.3**	41.0
Welfare and Food Stamp Receipt in Month Prior to Enrollment[b/]				
None	1.6	27.1	-2.8	37.8
Some	-3.4	26.7	-21.7*	38.1
Dependents				
None	-0.1	26.8	-7.8	37.6
One or more	-0.1	28.8	-11.4	40.5
Months in Longest Job			#	
0	13.3**	23.1	18.2	30.0
1 - 12	-4.1	28.6	-15.7**	40.7
More than 12	-4.3	24.7	-21.2	37.0

TABLE VI.6 (Continued)

	Months 1 - 18		Months 1 - 27[a]	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Weeks Worked in Year Prior to Enrollment[c]				
0	-0.5	25.5	-13.7**	38.5
5	-0.2	26.3	-10.8**	38.2
10	0.0	27.1	-8.0**	37.8
Prior Drug Use				
Used drugs other than marijuana	-7.7	35.3	-10.4	46.0
Did not use any drug other than marijuana	2.0	24.6	-7.3	34.6
Prior Arrests[c]				
0	-1.8	25.6	-13.6**	37.9
4	1.6	28.3	-4.6	37.9
9	5.9	31.6	6.8	37.8
Months Since Incarceration	#			
Never incarcerated	-3.6	26.3	-9.4	34.4
12 or less	6.8	30.3	-23.2	58.5
More than 12	26.4**	28.5	22.7	58.8
Parole or Probation at Enrollment				
Not on parole or probation	0.0	24.1	-8.3	33.7
On parole or probation	-0.3	35.2	-7.8	48.6

NOTE: See note to Table III.3. Results for the 1- to 36-month period are not presented because of the limited sample size (79).

[a] The sample for this period includes people who completed baseline, 9-month, 18-month, and 27-month interviews.

[b] Welfare includes AFDC, General Assistance, and other welfare or welfare income for which respondents could not identify the source.

[c] These estimates of subgroup effects and means are based on a linear specification of the sample characteristic, evaluated at the specified points.

# Experimental-control differentials within this subgrouping for this time period differ significantly from one another.

* Statistically significant at the 10 percent level.
** Statistically significant at the 5 percent level.

D. CONCLUSIONS

There is some evidence that among those individuals who enrolled in the Supported Work programs prior to 1977 (and so were followed for at least 27 months after their enrollment), the program did tend to reduce their likelihood of participating in criminal activities, as evidenced by the lower incidence of arrests among experimentals relative to controls. The subgroups of youth for whom the crime results exhibit a pattern consistent with that for the employment results are those age 17 or 18 at enrollment and those who enrolled in the program earlier (and so were followed for a longer period of time). However, the sites where the relatively more favorable employment results were observed are not the same set as those where there is some indication that the programs may have reduced criminal activities, suggesting that the mechanism through which crime-related impacts might occur may relate more to the sociological theories than to the economic theories discussed in Chapter II. Thus, the lack of apparent program impacts for the later enrollees may be attributable to changes in the character of the Supported Work programs over time--for example, to their considerably larger average size. However, as previously noted, this apparent reduction in program impacts over time is at least partly attributable to the differential impacts among sites.

CHAPTER VII

CONCLUSION

The goal of Supported Work is to mitigate a number of factors thought to be related to the unusually high unemployment among that segment of the youth population that was the target of the Supported Work demonstration--school dropouts with limited or no prior work experience and often with a history of involvement in crime. The evidence from the National Supported Work Demonstration suggests that Supported Work can be expected to have short-run impacts on employment and, consequently, on dependence on public assistance. However, it does not appear to be successful in its central objective of improving the long-term employment prospects for disadvantaged youth.

Those youth who met the Supported Work eligibility criteria and applied to the program are among those who characteristically have the most serious employment problems.[1] About 60 percent of the sample are younger than 19, over one-third completed fewer than 10 years of schooling, and over one-fifth have never had a regular job. Of those who have held a regular job, their most recent job had ended, on average, more than 11 months ago. In addition, 57 percent reported having been arrested, and 38 percent reported having been convicted. Other characteristics which identify the youth as being particularly likely to have limited employment opportunities are that 91 percent are from black and other ethnic minority groups (among whom the national unemployment rate is about double the overall unemployment rate for youth), and about two-thirds of the group had been out of school for more than a year when they enrolled in the program. Thirty percent of them reported having been expelled from school or left school because of problems with the police. Few of these youth were married and supporting dependents, and nearly two-thirds were living with their parents at the time of enrollment.

---

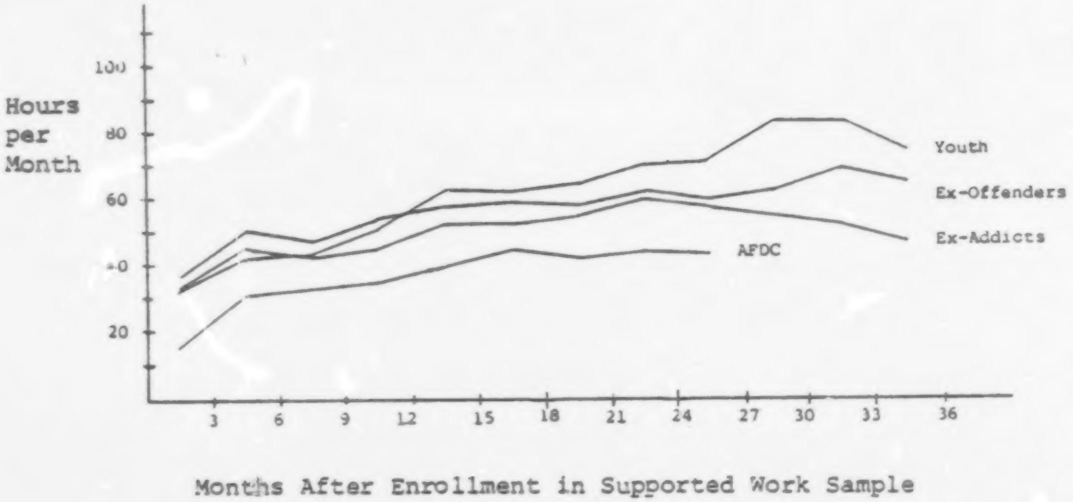
[1] For example, see Feldstein and Ellwood (1979) for a description of the employment problems of youth with various characteristics.

Based on the post-enrollment employment experiences of the youth control group, however, we observed that the employment prospects for these youth who applied to and were enrolled in the demonstration were somewhat more favorable than their background characteristics would lead one to expect and better than those of the other Supported Work target groups (see Figure VII.1). By the start of the third year after enrollment, half of the youth were employed and they worked an average of 70 hours per month. Eighty-three percent of the controls reported some employment during the follow-up period of 18 to 36 months. This general upward trend in employment is attributable to three factors: a normal tendency for some youth who, because of program eligibility requirements, were unemployed at enrollment to become employed, the aging of these youth, and improvements in local employment opportunities, due to both improving local labor-market conditions and an increase in CETA appropriations targeted for youth jobs.

Employment experiences of youth controls were considerably more favorable among those enrolled later in calender time, among those

FIGURE VII.1

TREND IN HOURS WORKED BY CONTROL GROUP MEMBERS



in Jersey City and Atlanta as compared with those in other sites, among males as compared with females, and among those with more as opposed to less prior work experience. Other characteristics, including age, showed only weak relationships with employment.

Those youth applicants who were randomly assigned to the experimental group, and were thus offered a Supported Work job, stayed in these jobs only 6.7 months, on average, even though under program guidelines they were permitted to stay much longer. Only 9 percent left after having exhausted the allowable time in the program; yet, only 18 percent left to take another job.

During the period when experimentals were employed in Supported Work, their hours of work and earnings, of course, exceeded those of controls by a significant amount. Consequently, their dependence on welfare decreased at the same time that their economic status improved.

Although nearly 40 percent of the experimental youth reported that Supported Work had helped prepare them for unsubsidized employment, largely by teaching job skills, the post-program employment experience of experimentals do not reflect such increases in employment skills: by the start of the second year when less than 20 percent of the experimentals were still in the program, there was essentially no difference in the overall employment level of the two groups. While differentials in employment rates did reappear during the latest follow-up period, they are neither large nor statistically significant. Furthermore, the estimated earnings differential during this same period is negative, implying that employed experimentals earned substantially lower wage rates than did employed controls.

Perhaps the most noteworthy factor concerning these employment results is that both experimentals and controls exhibited a reasonably favorable pattern of employment: during months 19 to 36, between 61 and 74 percent of the sample reported employment during each 9-month period, and those with some employment worked the equivalent of about two-thirds time at wage rates averaging between $3.41 and $4.13 per hour.

In comparison with other Supported Work target groups, youth experimentals had employment rates during the 19- to 36-month period that averaged 9 to 25 percent higher and they worked between 12 and 25 percent more hours (though at substantially lower average wage rates). Thus, that this group exhibited less employment gain vis-a-vis their control counterparts than other target groups appears to be due to their having a less chronic problem at the time of their enrollment, as evidenced by the previously noted more favorable employment experiences of their control group relative to those of other target groups. This conclusion is further supported by the observation that those youth for whom relatively more favorable (though generally not significant) patterns of effects were estimated are those whose control group counterparts had low employment rates and levels--for example, the earliest enrollees in the demonstration who faced the poorest labor-market conditions, those in New York, Hartford, and Philadelphia, and those who were younger than average. However, the findings of this study provide little evidence that retargeting the program on a different subset of youth would substantially alter the program's impacts.

Experimental youth did stay in Supported Work longer than controls over the same period stayed in nonprogram jobs (6.7 months versus 5.6 months for nonprogram jobs). However, longer tenure in Supported Work jobs has not been found to result in improvements in other dimensions of employment-related outcomes, such as employment rates, employment levels, or wage rates. Similarly, the Supported Work experience did not lead to substantially different types of jobs; among both experimentals and controls, two-thirds to three-quarters of their nonprogram jobs were in the manufacturing, retail trade, and service industries, and they were mainly in clerical, service and miscellaneous occupations.

Among the reasons for public concern with the high rates of youth unemployment is the belief that unemployment contributes to drug abuse and criminal behavior among young people. Yet, this study provides no support for such beliefs. Even during the first nine months after enrolling in the demonstration when experimentals had been offered a program job, the extent of drug use and involvement in crime were similar between experimentals and controls: roughly 13 percent reported using drugs (other than marijuana or alcohol) and 17 percent were arrested.

As a consequence of these limited program impacts, the estimates of the net social costs of Supported Work for youth are high: costs are estimated to exceed benefits by an average of about $1,465 per participant. Thus, a decision as to whether or not Supported Work has a place among federally sponsored youth employment programs is a matter of judgment as to the reliability of this estimate of the required net subsidy and the unmeasured social value of achieving relatively modest short-term employment and income gains.

In arriving at such judgments about Supported Work, it is important to recognize the limitations of the demonstration's findings. The most serious limitation is, of course, the paucity of observations on which to base estimates of long-term impacts. However, another failing is that the research did not explore fully the "causes" of the program's apparent lack of success in improving longer-term employment prospects for the target population. While the current data base would support a more thorough analysis of the causes of youth unemployment and of Supported Work's limited achievements, the reliability of the estimates of longer-term impacts could be improved only through further follow-up of the sample.

APPENDIX A

SUPPLEMENTARY TABLES

TABLE A.1

CHARACTERISTICS OF EXPERIMENTAL AND CONTROL GROUP MEMBERS AT ENROLLMENT

YOUTH SAMPLE

Characteristic	Experimental Group Mean	Experimental-Control Differential
Age in Years	18.22	-0.04**
Proportion Male	0.88	0.01
Proportion Black	0.74	0.03
Number of Dependents	0.18	0.04
Number of Years of Formal Education	9.62	-0.06
Number of Weeks Worked Last 12 Months	9.45	-0.09
Average Wage for Those Employed Last 12 Months	2.63	-0.17
Unearned Income Last 4 Weeks	56.14	-2.43
Proportion Receiving Welfare	0.13	-0.01
Number of Arrests	1.99	-0.67**
Number of Convictions	0.52	-0.23**
Proportion Ever Used Drugs[a]	0.23	-0.02
Proportion Ever Used Heroin	0.07	-0.01

NOTE: These data pertain to the total youth sample and are taken from Jackson et al. (1978), Table B-3.

[a] This includes only individuals who have used drugs other than marijuana and alcohol.

**Significant at the .05 level on a two-tailed test.

TABLE A.2

SAMPLE SIZES FOR ANALYSIS OF VARIOUS OUTCOME MEASURES IN VARIOUS TIME PERIODS

	Months Covered by Outcome Measure				
Outcome Measures	1-18	19-27	28-36	1-27	1-36
Employment	849	508	153	n.a.	n.a.
Income Sources and Welfare Dependence	643	460	149	n.a.	n.a.
Use of Drugs (other than Marijuana)	852	507	151	n.a.	n.a.
Use of Marijuana, Enrollment in Drug Treatment, Drug Use Among Sample Subgroups	739	497	151	n.a.	n.a.
Daily Use of Marijuana, Index of Drug Use	733	488	143	n.a.	n.a.
Indicators of Criminal Activities	809	507	146	379	79
Total Potential Sample[a]	861	513	153	419	121

[a] This includes all individuals who completed the required interviews: the potential sample for 1- to 18-month outcomes includes all who completed an enrollment, a 9-month, and an 18-month interview; that for 19- to 27-month outcomes includes all who completed an enrollment and a 27-month interview; that for months 28 to 36 includes all who completed an enrollment and a 36-month interview; those for months 1 to 27 and 1 to 36 include all who completed all scheduled interviews up to the 19- to 27- and the 28- to 36-month interview, respectively.

n.a. means not applicable.

TABLE A.3

NUMBERS OF INTERVIEWS ASSIGNED AND COMPLETED

Interview Type	Number Assigned	Number Completed[a]	Percentage Completed
Enrollment	1252	1244	99.4
9-month	1252	1001	80.0
18-month	1252	924	73.8
27-month	719[b]	506	70.4
36-month	202[c]	155	76.7

NOTE: These data are from Jackson et al. (1979), Tables II.1 and VI.A.2-VI.A.4.

[a] These figures include three persons who completed a substitute enrollment interview at the time of a subsequently scheduled follow-up interview. They do not include individuals who completed substitute follow-up interviews.

[b] Only those enrolled prior to January 1977 were assigned a 27-month interview.

[c] Only those enrolled prior to April 1976 were assigned a 36-month interview.

TABLE A.4a

MEANS OF CONTROL VARIABLES USED IN REGRESSIONS

(Standard Deviations of Continuous Variables are in Parentheses)

	Sample		
	1-18 Month Outcomes	19-27 Month Outcomes	28-36 Month Outcomes
Experimental Group	0.478	0.472	0.497
Amount of Follow-up Data[a]			
27 Months	0.488	--	--
36 Months	0.147	0.283	--
Pressured by Welfare, Drug Treatment or Criminal Justice Agency to Apply to Program[a]	0.065	0.099	0.085
Site			
Atlanta	0.093	0.026	0.0
Hartford	0.446	0.453	0.131
Jersey City	0.225	0.333	0.555
New York	0.159	0.035	0.0
Philadelphia	0.077	0.154	0.314
Months of Program Operation			
13-18	0.332	0.419	0.0
> 18	0.342	0.142	0.0
Area Unemployment Rate During Follow-Up Period[b]	7.490 (2.684)	7.372 (2.981)	8.913 (2.281)
Complies with Formal Program Eligibility Criteria	0.732	0.719	0.599
Age 19 or older	0.403	0.414	0.430
Male	0.862	0.892	0.941
Race/Ethnicity			
White, non-Hispanic	0.060	0.092	0.071
Hispanic	0.155	0.147	0.121
More than 8 Years of School	0.848	0.835	0.817
Time Since Last Enrolled in School[a]			
1 Year	0.367	0.340	0.404
1-2 Years	0.267	0.301	0.244
Reason Left School[a]			
Expelled	0.155	0.159	0.111
Trouble with Law	0.145	0.177	0.222
Wanted a Job	0.287	0.288	0.353
Number of Persons in Household	4.923 (2.563)	5.059 (2.387)	4.979 (2.571)
Any Dependents	0.102	0.086	0.058
Raised by[a]			
One Parent	0.564	0.529	0.444
Two Parents	0.347	0.400	0.477
Currently Living with Parents[a]	0.701	0.717	0.760
Any Food Stamps or Welfare Last Month	0.343	0.304	0.255
Total Income Last Month[c]	120.15 (132.74)	122.37 (140.87)	127.95 (129.13)
Earnings Last Month[c]			
Any	0.572	0.605	0.638
Amount	67.24 (98.44)	74.00 (104.04)	87.25 (105.30)

TABLE A.4a (CONTINUED)

MEANS OF CONTROL VARIABLES USED IN REGRESSIONS

(Standard Deviations of Continuous Variables are in Parentheses)

	Sample		
	1-18 Month Outcomes	19-27 Month Outcomes	28-36 Month Outcomes
Unemployment Compensation Last Month[c]			
Any	0.028	0.035	0.034
Amount	5.08	6.37	8.87
	(33.60)	(37.20)	(50.22)
Welfare last Month[c]			
Any	0.13[illegible]	0.122	0.114
Amount	22.30	19.83	16.48
	(67.34)	(64.02)	(49.63)
Food Stamps last Month[c]			
Any	0.252	0.205	0.149
Bonus Value	20.04	15.46	7.81
	(38.80)	(36.15)	(18.26)
Other Unearned Income Last Month[c]			
Any	0.041	0.049	0.065
Amount	5.49	6.71	7.54
	(31.35)	(36.93)	(40.13)
Weeks Worked in Prior Year	9.359	10.172	12.659
	(11.684)	(12.362)	(14.013)
Length of Longest Job Ever			
12 months or less	0.696	0.687	0.687
More than 12 months	0.075	0.076	0.099
8 or More Weeks of Job Training Prior Year[d]	0.109	0.107	0.139
Used Any Drugs (Other than Marijuana)	0.233	0.300	0.411
Used Cocaine[c]	0.129	0.150	0.187
Used Alcohol Daily[c]	0.058	0.072	0.077
Best Friend Does Not Use Drugs and is Not Involved in Crime[e]	0.814	0.753	0.715
Many Addicts in Neighborhood[e]	0.323	0.352	0.340
Ever Arrested	0.542	0.638	0.641
Number of Arrests	2.250	2.868	3.000
	(5.305)	(5.024)	(5.225)
Time since Incarcerated			
12 months or less	0.174	0.191	0.204
More than 12 months	0.112	0.199	0.303
On Parole or Probation[e]	0.275	0.321	0.316
Maximum Number of Cases in Regressions	861	513	153

NOTE: Means of these variables will vary slightly from one set of regressions to another because of slightly different sample sizes for analyses of various outcome measures. They may also vary from those presented in Table II.3 because the above means were obtained from actual analysis samples as opposed to potential samples based on interview completions.

[a] These variables were included only in regressions to estimate subgroup effects for individuals with the various attributes.

[b] Area unemployment rate was ultimately excluded from regressions because of its high correlation with the site variables. The 1-18 month value pertains to months 10-18. The value for months 1-9 is 8.62 percent.

[c] These variables were included only in regressions where the dependent variable was the post-enrollment value of the same.

[d] This variable was included only in employment-related regressions.

[e] This variable was included only in regressions where indicators of drug use and criminal activities were the dependent variables.

TABLE A.4b

ESTIMATED COEFFICIENTS ON CONTROL VARIABLES
USED IN SELECTED REGRESSION EQUATIONS

	Dependent Variable		
Control Variable	Hours Employed Per Month (Months 19 to 27)[a]	Used Any Drugs (Months 19 to 27)[b]	Any Arrest (Months 19 to 27)[c]
Amount of Follow-up Data			
27 Months	n.a.	n.a.	n.a.
36 Months	n.a.	n.a.	n.a.
Site			
Atlanta	42.11	-0.12	-0.09
Hartford	-23.36 **	-0.11	0.01
Jersey City	n.a.	n.a.	n.a.
New York	-53.07 *	-0.03	-0.04
Philadelphia	-40.53 **	-0.15	0.06
Months of Program Operation			
13-18	14.87 *	-0.03	-0.08*
> 18	19.90 *	-0.03	-0.10
Complies with Formal Program Eligibility Criteria	1.06	0.04	-0.06
Age 19 or Older	14.03 **	-0.02	-0.07**
Male	43.50 **	-0.01	0.13**
Race/Ethnicity			
White, Non-Hispanic	33.20 **	0.09*	0.04
Hispanic	17.18 *	0.01	0.00
More than 8 Years of School	15.18 *	0.05	0.05
Number of Persons in Household	0.28	-0.00	-0.01
Any Dependents	-18.52	0.00	0.05
Any Food Stamps or Welfare Last Month	-18.36 **	0.02	-0.03
Weeks Worked in Prior Year	0.72 **	-0.00	-0.00
Length of Longest Job Ever			
12 Months or Less	-2.42	0.02	-0.07*
More Than 12 Months	9.67	-0.06	-0.07
8 or More Weeks of Job Training Prior Year	-0.93	n.a.	n.a.
Used Any Drugs (Other Than Marijuana)	-4.48	0.16**	0.02
Ever Arrested	-15.64 *	0.03	0.02
Number of Arrests	-0.32	-0.00	0.00
Time Since Incarcerated			
12 Months or Less	-12.93	0.02	-0.01
More Than 12 Months	16.42	-0.01	-0.06
On Parole or Probation	n.a.	n.a.	0.00
Constant	24.26	-0.02	0.19
Number of Cases	508	507	507
$\bar{R}^2$	0.14	0.09	0.03

[a] This equation also included five binary variables indicating experimental status and site (i.e., status*Atlanta, . . ., status*Philadelphia). Program effects estimated from this equation are reported in Table III.7.

[b] This equation also included five binary variables indicating experimental status and site (i.e., status*Atlanta, . . ., status*Philadelphia). Program effects estimated from this equation are reported in Table V.3.

[c] This equation included two binary variables indicating experimental status and amount of follow-up data (i.e., status*27 months and status*36 months). Program effects estimated from this equation are reported in Table VI.3.

n.a. means not applicable

* Statistically significant at the 10 percent level, two-tailed test.
** Statistically significant at the 5 percent level two tailed test.

TABLE A.5

ESTIMATED REGRESSION COEFFICIENTS ON VARIABLES USED TO PREDICT WEEKS WORKED BY CONTROLS DURING THE FIRST 18 MONTHS FOLLOWING ENROLLMENT

(Omitted variables in parentheses)

Variable	Estimated Coefficient	t-ratio	Significance Level (%)
Atlanta	-1.66	-0.36	72
Hartford	-11.12	-3.66	0
(Jersey City)			
New York	-9.77	-2.45	1
Philadelphia	-12.81	-3.06	0
(Enrolled Before July 1976)			
Enrolled July-December 1976	6.20	2.19	3
Enrolled 1977	7.82	2.54	1
(Age ≤ 17)			
Age 18	1.34	0.48	63
Age 19	2.54	0.81	42
Age ≥ 20	2.45	0.69	49
Male	7.14	2.18	3
(Female)			
White	-1.28	-0.29	78
Hispanic	4.27	1.46	14
(Black and Other)			
(<10 Years of School)			
10 Years of School	-1.83	-0.76	45
>10 Years of School	2.14	0.77	44
<1 Year Since School	2.93	1.13	26
1-2 Years Since School	1.39	0.52	60
(>2 Years Since School)			
Expelled From School or Left Because of Trouble With Law	-2.11	-0.94	35
(Left School for Other Reasons)			
Lives with Parents	-3.41	-1.38	17
(Does not Live with Parents)			
Raised by Two Parents	-2.60	-1.21	23
(Not raised by Two Parents)			
Married and/or Has Dependents	-2.07	-0.58	57
(Not Married and No Dependents)			
Receiving Welfare or Food Stamps	-2.73	-1.06	29
(Not Receiving Welfare)			
(No Previous Regular Job)			
Longest Regular Job Lasted <6 Months	1.76	0.69	49
Longest Regular Job Lasted ≥6 Months	5.57	1.74	8
(No Job Training in Past Year)			
Some Job Training in Past Year	0.86	0.28	78
(Never Used Any Drugs)			
Used Only Marijuana	-1.56	-0.65	52
Used Drugs Other than Marijuana	-0.65	-0.22	82
(Never Arrested)			
One or More Arrests	-1.96	-0.78	44
(Not on Parole or Probation)			
On Parole or Probation	-0.83	-0.33	74
Constant	20.02	3.52	0
Average Number of Weeks Worked	22.16		
$\bar{R}^2$	0.06		
Number of Observations	446		

TABLE A.6

ESTIMATED REGRESSION COEFFICIENTS ON VARIABLES USED TO PREDICT WEEKS EXPERIMENTALS WORKED IN SUPPORTED WORK JOBS

(Omitted variables in parentheses)

Variable	Estimated Coefficient	t-ratio	Significance Level (%)
Atlanta	-2.27	-0.53	60
Hartford	-6.50	-2.11	4
(Jersey City)			
New York	1.37	0.34	73
Philadelphia	-19.82	-4.59	0
(Enrolled Before July 1976)			
Enrolled July-December 1976	-5.13	-1.77	8
Enrolled 1977	-5.48	-1.71	9
(Age ≤ 17)			
Age 18	-2.06	-0.75	46
Age 19	-0.88	-0.26	78
Age ≥ 20	-1.31	-0.33	74
Male	-5.83	-1.71	9
(Female)			
White	-1.99	-0.43	67
Hispanic	2.28	0.73	46
(Black and Other)			
(< 10 Years of School)			
10 Years of School	5.94	2.44	2
> 10 Years of School	3.58	1.22	22
< 1 Year Since School	-0.16	-0.06	95
1-2 Years Since School	2.13	0.78	43
(> 2 Years Since School)			
Expelled From School or Left Because of Trouble With Law	-3.37	-1.41	16
(Left School for Other Reasons)			
Lives with Parents	0.84	0.35	73
(Does not Live with Parents)			
Raised by Two Parents	4.16	1.93	5
(Not raised by Two Parents)			
Married and/or Has Dependents	-5.50	-1.56	12
(Not Married and No Dependents)			
Receiving Welfare or Food Stamps	2.70	1.03	30
(Not Receiving Welfare)			
(No Previous Regular Job)			
Longest Regular Job Lasted < 6 Months	5.01	1.95	5
Longest Regular Job Lasted ≥ 6 Months	4.17	1.33	18
(No Job Training in Past Year)			
Some Job Training in Past Year	8.40	2.91	0
(Never Used Any Drugs)			
Used Only Marijuana	2.77	1.14	26
Used Drugs Other than Marijuana	1.53	0.53	60
(Never Arrested)			
One or More Arrests	-2.46	-0.96	34
(Not on Parole or Probation)			
On Parole or Probation	1.10	0.39	69
Constant	33.12	5.99	0

Average Number of Weeks Worked	29.31
$\bar{R}^2$	0.11
Number of Observations	404

TABLE A.7

ESTIMATES OF MARGINAL IMPACTS OF VARIOUS CHARACTERISTICS ON THE LIKELIHOOD OF TERMINATING FROM SUPPORTED WORK FOR DIFFERENT REASONS

YOUTH EXPERIMENTAL SAMPLE

	Reason for Termination		
	To Take Another Job or to Enroll in School or Job Training	Poor Performance[a]	Other
Atlanta	0.03	0.04	-0.07
Hartford	0.04	0.09	-0.13
(Jersey City)			
New York	-0.06	0.29*	-0.22
Philadelphia	0.27	0.23	-0.50
(Enrolled Before July 1976)			
Enrolled July-December 1976	-0.18**	-0.06	-0.24
Enrolled 1977	-0.17*	-0.13	0.31
(Age ≤ 17)			
Age 18	-0.01	0.11	-0.10
Age 19	-0.00	0.13	-0.13
Age ≥ 20	-0.15	0.11	0.05
Male	0.05	-0.01	-0.01
(Female)			
White	0.01	-0.00	0.01
Hispanic	0.01	0.01	0.06
(Black and Other)			
(< 10 Years of School)			
10 Years of School	0.14**	-0.14	0.00
> 10 Years of School	0.10	-0.04	-0.13
< 1 Year Since School	0.04	-0.11	-0.07
1-2 Years Since School	-0.04	-0.08	0.12
(> 2 Years Since School)			
Expelled from School or Left Because of Trouble with Law	-0.15**	0.16	-0.01
(Left School for Other Reasons)			
Lives with Parents	0.08	-0.09	0.02
(Does Not Live with Parents)			
Raised by Two Parents	0.01	-0.02	0.02
(Not Raised by Two Parents)			
Married and/or Has Dependents	-0.01	-0.01	0.02
(Not Married and No Dependents)			
Receiving Welfare or Food Stamps	-0.01	-0.10	0.11
(Not Receiving Welfare)			
(No Previous Regular Job)			
Longest Regular Job Lasted < 6 Months	0.05	-0.04	0.01
Longest Regular Job Lasted ≥ 6 Months	0.12	-0.24*	0.12
(No Job Training in Past Year)			
Some Job Training in Past Year	0.03	-0.12	0.10
(Never Used Any Drugs)			
Used Only Marijuana	0.06	-0.04	-0.03
Used Drugs Other than Marijuana	-0.02	0.14	-0.12
(Never Arrested)			
One or More Arrests	0.02	0.08	-0.11
(Not on Parole or Probation)			
On Parole or Probation	-0.02	-0.01	-0.03
Constant	-0.20	0.24	-0.04
Percentage of Sample	19.17	44.36	36.47

NOTE: Samples used are defined in Table III.1. These estimates of marginal impacts are based on polytomous logit analysis which predicted accurately reasons for termination of 55 percent of the sample.

[a] This category includes those who terminated because of conflicts with the boss or crew members, use of drugs or alcohol, illegal activities or incarceration, absenteeism, poor punctuality, or low productivity.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.8a

PERCENTAGE OF EXPERIMENTALS REPORTING VARIOUS ASSESSMENTS OF SUPPORTED WORK

YOUTH SAMPLE

	Months in Supported Work Job			
	Less than 3	3 to 12	12 or more	Total
Supported Work Prepared Him/Her to Obtain Regular Job	23.5	36.7	55.3	38.2
Prepared Him/Her by Teaching:[a]				
Job skills, trade	62.5	80.0	66.7	73.7
Better work habits and attitudes	31.3	40.5	19.0	32.6
Other	12.5	10.1	21.4	13.9
Most Important Result from Working in Supported Work is:				
Learning jobs skills, trade	17.2	27.8	42.1	29.0
Developing better work habits and attitudes	9.4	10.1	7.9	9.5
Having a steady job and income	20.3	16.7	17.1	17.5
Developing self-confidence, self-esteem	9.4	5.6	9.2	7.1
Staying out of trouble and/or off drugs	3.3	2.0	0.0	1.8
Other things	12.5	22.7	21.4	19.8
Nothing	4.5	36.9	23.7	35.5
There Were Things He/She Did Not Like About Supported Work	49.6	38.1	31.6	37.1
How Program was run[b]	28.0	30.1	4.2	25.0
Low pay[b]	26.9	17.3	45.8	24.8
Other complaints[b]	46.2	57.5	54.2	54.6
Number in Sample	114	374	83	571

[a] Figures include only those who said Supported Work prepared them to obtain a regular job. Multiple responses were permitted.

[b] Figures include only those who said there were things they did not like about Supported Work. Multiple responses were permitted.

TABLE A.8b

PERCENTAGE OF EXPERIMENTALS REPORTING VARIOUS REASONS WHY SUPPORTED WORK DIFFERED FROM OTHER JOBS

YOUTH SAMPLE

	Months in Supported Work Job			
	Less than 3	3 to 12	12 or more	Total
Job Skills and Attitudes				
Learn new skills or trade	16.2	25.2	18.8	22.4
Develop better work attitudes	0.0	2.1	0.0	1.3
Develop self-confidence	0.0	1.4	0.0	0.9
Type of Work				
More enjoyable work	8.1	2.1	4.2	3.5
Less enjoyable work	0.0	0.7	0.0	0.4
Different kind of work	40.5	35.0	27.1	34.2
Easier work	5.4	4.2	10.4	5.7
Harder work	8.1	2.1	4.2	3.5
Program/Supervisor				
More interest in individual	2.7	0.0	0.0	0.4
More supervision	2.7	6.3	4.2	5.3
Less supervision	2.7	0.0	0.0	0.4
More lenient supervision	0.0	2.8	0.0	1.8
Program run poorly	10.8	8.4	4.2	7.9
Program run better	0.0	1.4	2.1	1.3
Program run differently	0.0	5.6	6.3	4.8
Liked those running program	0.0	0.7	2.1	0.9
Did not like those running program	5.4	4.9	3.1	4.4
Fellow Workers				
Likes fellow workers	2.7	0.0	2.1	0.9
Does not like fellow workers	2.7	2.1	4.2	2.6
Wages and Working Conditions				
High wage rates	0.0	0.0	0.0	0.0
Low wage rates	2.7	2.8	6.3	3.5
Poorer benefits	0.0	0.7	2.1	1.8
Better benefits	5.4	0.7	2.1	1.7
Poorer working conditions	0.0	1.4	0.0	0.9
Longer hours	0.0	0.7	0.0	0.4
Other	5.4	0.7	2.1	2.2
(Supported Work Not Different)	(40.3)	(26.7)	(32.3)	(30.5)
Number in Sample	37	143	48	228

NOTE: Of the experimentals, 3 percent were no-shows and 6 percent were not in the program 30 days. Multiple responses were allowed. Figures include only those who said that Supported Work differed from other jobs.

TABLE A.9

PERCENTAGE WITH POST-PROGRAM JOB, BY REASON FOR LEAVING SUPPORTED WORK AND AMOUNT OF POST-SUPPORTED WORK FOLLOW-UP DATA

(For Those With Job, Average Number of Months to First Post-program Job in Parentheses)

Reason for Leaving Supported Work	Amount of Post-Supported Work Follow-Up Data				Total Sample
	≤ 6 months	7 to 12 months	13 to 18 months	> 18 months	
Exhausted Allowable Time in Program[a]	55.6 (0.6)	100.0 (0.0)	100.0 (4.5)	100.0 (7.5)	80.0 (3.7)
To Take Another Job or Job Training	60.0 (0.3)	100.0 (0.2)	91.3 (0.5)	94.7 (3.5)	90.6 (1.6)
Poor Performance[b]	33.3 (2.0)	75.0 (3.4)	64.7 (5.2)	69.2 (6.5)	67.5 (5.4)
Other[c]	86.7 (1.1)	47.1 (2.6)	62.5 (3.9)	73.3 (7.5)	65.5 (3.8)

NOTE: The amount of post-Supported Work follow-up data is the number of months between the time the sample member left the Supported Work job and the date of the latest month of continuous follow-up data.

[a] This includes individuals not leaving Supported Work to take another job, to enroll in school or job training, or because of poor performance, but who either spent the maximum number of months in the program or exceeded the maximum calendar time for participation.

[b] This category includes those terminated because of conflicts with the boss or crew members, use of drugs or alcohol, illegal activities or incarceration, absenteeism, poor punctuality, or low productivity.

[c] This includes reasons such as low pay and health, childcare, or transportation problems.

TABLE A.10

PERCENTAGE DISTRIBUTION OF SAMPLE BY AVERAGE NUMBER OF HOURS WORKED AND AVERAGE EARNINGS PER MONTH

YOUTH SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimentals	Controls	Experimentals	Controls	Experimentals	Controls	Experimentals	Controls
Hours Worked per Month								
0	2.0	47.0	31.6	40.1	38.2	39.2	26.3	33.8
1-43	10.0	17.8	11.2	16.6	12.4	10.1	11.8	7.8
44-86	17.3	15.4	17.8	13.7	11.6	13.1	17.1	10.4
87-129	18.0	11.6	13.1	11.4	8.3	13.8	13.2	11.7
130-172	27.6	4.5	15.1	7.2	14.1	7.8	5.3	15.6
173-216	24.1	3.3	10.5	8.1	12.4	13.1	22.4	19.5
217-259	0.7	0.2	0.7	1.8	1.7	1.1	2.6	0.0
≥ 260	0.2	0.2	0.0	1.1	1.2	1.9	1.3	1.3
(Average hours)	(120.5)	(39.6)	(70.6)	(51.7)	(68.9)	(68.5)	(87.5)	(82.5)
Earnings per Month (dollars)								
0	2.0	47.2	31.7	40.1	38.2	38.9	26.3	33.8
1-99	9.3	16.1	8.3	13.5	9.1	7.4	7.9	5.2
100-199	13.9	13.4	14.4	11.7	8.3	10.4	15.8	5.2
200-299	13.0	9.2	10.7	9.2	5.8	6.7	5.3	6.5
300-399	16.9	5.8	9.3	5.6	5.8	10.0	7.9	5.2
400-499	32.3	3.4	9.5	6.7	9.5	7.4	9.2	10.4
500-599	9.8	3.8	7.8	5.2	5.4	5.9	10.5	11.7
600-699	1.2	0.4	5.4	3.4	9.1	5.9	7.9	5.2
700-799	0.7	0.2	1.5	1.6	3.7	1.5	0.0	7.8
800-899	1.0	0.0	0.5	1.6	1.7	1.9	5.3	5.2
≥ 900	0.0	0.4	1.0	1.6	3.3	4.1	3.9	3.9
(Average dollars)	(340.08)	(124.39)	(232.32)	(193.76)	(263.33)	(249.65)	(301.50)	(3[illegible]6.76)
Number in Sample	409	447	410	446	241	270	76	77

NOTE: Samples used are defined in Table II.2. These data are not regression-adjusted. Columns may not sum to 100 due to rounding.

TABLE A.11

COMPONENTS OF EXPERIMENTAL-CONTROL DIFFERENTIAL IN HOURS EMPLOYED PER MONTH

YOUTH SAMPLE

(Dollars)

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Value	Percent of Total	Value	Percent of Total	Value	Percent of Total	Value	Percent of Total
Overall Differential	85.04**	100.0	12.17**	100.0	2.00	100.0	11.51	100.0
Differential Due to								
Change in probability of employment	45.11	53.05	6.79	55.79	1.09	54.50	5.28	45.87
Change in hours worked among employed	39.93	46.95	5.38	44.21	0.91	45.50	6.23	54.13

NOTE: The decomposition of the overall differential was estimated from a tobit equation in which the overall differential, $\Delta E(Y)$, can be expressed as:

$$X_E\beta F_E(\cdot) + \sigma f_E(\cdot) - X_c\beta F_c(\cdot) - \sigma f_c(\cdot).$$

where: X is a vector of control variables; β is a vector of estimated coefficients; $F(\cdot)$ denotes the cumulative normal distribution evaluated at X; $f(\cdot)$ denotes the probability density function evaluated at X; σ is the standard error of the equation; and E and c denote experimentals' and controls' values, respectively.

The two components, respectively, can be expressed as:

$$E(Y_E)^* \left[F_E(\cdot) - F_c(\cdot)\right]$$

and

$$\Delta E(Y)^* F_c(\cdot)$$

See McDonald and Moffitt (forthcoming) for a discussion of this decomposition procedure.

TABLE A.12

AVERAGE HOURLY WAGE RATE

YOUTH SAMPLE

(dollars)

Months	Experimental Group Mean All Jobs	Experimental Group Mean Non-Supported Work Jobs	Control Group Mean
1-3	2.72	2.92	3.21
4-6	2.84	3.09	3.20
7-9	2.93	3.28	3.09
10-12	3.21	3.65	3.26
13-15	3.26	3.39	3.39
16-18	3.46	3.50	3.48
19-21	3.82	3.80	3.47
22-24	3.89	3.89	3.66
25-27	3.85[a]	3.86	3.81
28-30	3.45	3.45	3.90
31-33	3.49	3.49	4.23
34-36	3.45	3.45	4.37

NOTE: These wage rates are calculated by dividing the average monthly earnings of sample members (Table III.5) by their average monthly hours (Table III.4). No significance tests were computed.

[a] No individuals should have been in Supported Work during this time period and thus wage rates on all jobs should equal those on non-Supported Work jobs. That these two wage rates are not equal is due either to misreporting of hours and/or earnings or to an occasional failure for individuals to be terminated on schedule.

TABLE A.13

AVERAGE NUMBER OF HOURS WORKED PER MONTH FOR SUBSAMPLES WITH VARIOUS AMOUNTS OF FOLLOW-UP DATA

YOUTH SAMPLE

Months After Enrollment	18 Months of Data			27 Months of Data			36 Months of Data		
	Experimentals	Controls	Experimental-Control Differential	Experimentals	Controls	Experimental-Control Differential	Experimentals	Controls	Experimental-Control Differential
1-3	136.45	25.69	110.76	148.13	32.95	115.18	153.41	39.76	113.65
4-6	117.36	45.87	71.49	121.72	39.88	81.84	125.00	45.52	79.48
7-9	94.01	51.25	42.76	101.05	37.02	64.03	98.00	42.60	55.40
10-12	81.54	60.09	21.45	74.06	40.44	33.62	82.66	25.17	57.49
13-15	72.27	66.43	5.84	61.50	63.20	-1.70	72.58	44.17	28.41
16-18	72.80	66.45	6.35	53.66	65.51	-11.85	52.11	34.92	17.19
19-21	n.a.	n.a.	n.a.	63.05	63.55	-0.50	57.43	66.02	-8.59
22-24	n.a.	n.a.	n.a.	70.24	65.95	4.29	69.88	71.98	-2.10
25-27	n.a.	n.a.	n.a.	72.19	65.75	6.44	66.96	81.96	-15.00
28-30	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	97.40	86.05	11.35
31-33	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	97.63	90.02	7.61
34-36	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	89.23	81.74	7.49
Number in Sample			436			298			121

NOTE: Samples are defined by the length of post-enrollment period for which continuous data are available. These data are not regression-adjusted. Thus, results differ somewhat from those reported elsewhere in the report. No significance tests were computed.

n.a. means not applicable.

TABLE A.14

CETA, WIN, AND OTHER PUBLIC SECTOR EMPLOYMENT

YOUTH SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Percentage Reporting Any CETA or WIN job	-2.4	3.7	0.1	4.1	1.5	4.9	-1.2	7.9
Average Monthly Earnings from CETA or WIN Job (dollars)	-4.23	7.57	3.30	8.56	3.71	15.63	-33.96	55.70
Percentage Reporting Any Government Job[a]	4.7	8.4	3.6	14.0	1.1	11.9	-11.7	21.1
Average Monthly Earnings from All Government Jobs[a] (dollars)	-9.06	17.36	-0.01	38.25	12.49	39.54	-82.85	120.03

NOTE: These data are not regression-adjusted. No tests of statistical significance were computed. Jobs were categorized according to sample members' responses to questions about whether specific jobs were part of special employment programs like CETA or WIN and whether they were for state or local governments.

[a] These figures include CETA and WIN jobs.

TABLE A.15

AVERAGE MONTHLY EARNINGS--TOTAL, EXCLUDING CETA AND WIN EARNINGS, AND EXCLUDING ALL PUBLIC SECTOR EARNINGS--BY AMOUNT OF FOLLOW-UP DATA

YOUTH SAMPLE
(dollars)

Months After Enrollment	18 Months of Data		27 Months of Data		36 Months of Data	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
A. Total Earnings						
16-18	13.24	235.34	-7.34	207.08	26.74	128.16
19-27	n.a.	n.a.	22.34	244.48	-41.26	286.80
28-36	n.a.	n.a.	n.a.	n.a.	-22.84	351.84
B. Earnings Net of CETA and WIN Earnings						
16-18	9.04	224.39	-7.78	194.52	16.79	128.16
19-27	n.a.	n.a.	18.09	225.61	-34.77	271.98
28-36	n.a.	n.a.	n.a.	n.a.	15.08	287.52
C. Earnings Net of All Public Sector Earnings						
16-18	3.79	194.98	-5.45	157.83	18.57	99.37
19-27	n.a.	n.a.	-3.24	204.67	-25.26	239.82
28-36	n.a.	n.a.	n.a.	n.a.	75.94	207.87

NOTE: Samples are defined by length of post-enrollment period for which continuous data are available and the data are not regression-adjusted. Thus, results differ somewhat from those reported elsewhere in the report. See Table A.14 for definitions of employment sectors. No significance tests were computed.

n.a. means not available.

TABLE A.16

TOBIT ESTIMATES OF HOURS EMPLOYED PER MONTH

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Total Sample	85.04**	35.63	12.17**	55.15	2.00	60.96	11.51	71.63
Site								
Atlanta	82.23**	53.55	9.24	73.05	-18.03[a]	119.84[a]	n.a.	n.a.
Hartford	93.07**	28.95	11.44*	47.34	4.13	55.07	20.88[a]	68.68[a]
Jersey City	95.30**	43.78	-1.22	73.06	-11.44	82.41	-9.82	99.21
New York	62.59**	39.82	20.69*	61.64	12.53[a]	29.03[a]	n.a.	n.a.
Philadelphia	53.48**	27.07	32.13**	27.11	17.97	38.99	35.53*	34.57
Number in Sample	859		857		509		153	

NOTE: See note to Table III.7.

[a] Sample size is less than 20.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.17

PERCENTAGE HAVING CETA OR WIN JOBS, BY SITE

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Atlanta	-6.82	6.82	3.89	2.63	0.0[a]	0,0[a]	n.a.	n.a.
Hartford	-0.26	0.76	0.34	3.61	4.26	3.03	1.29[a]	15.38[a]
Jersey City	-6.71	7.69	-3.77	6.80	0.72	9.41	-5.45	10.00
New York	-2.36	5.97	1.00	3.23	-14.29[a]	14.29[a]	n.a.	n.a.
Philadelphia	-2.05	4.55	2.93	2.63	0.00	0.00	8.00	0.00
Total	-2.39	3.65	0.12	4.08	1.51	4.87	-1.22	7.89

NOTE: These data are based on simple subgroup means. No test statistics were computed.

[a] Sample size is less than 20.

n.a. = not applicable.

TABLE A.18

PERCENTAGE HAVING CETA, WIN, OR OTHER GOVERNMENT JOBS, BY SITE

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Atlanta	-5.23	11.11	12.69	18.42	33.33[a]	16.67[a]	n.a.	n.a.
Hartford	-0.91	3.37	-2.23	9.49	3.54	6.77	-6.41[a]	23.08[a]
Jersey City	-11.04	15.89	-17.88	25.96	0.07	21.18	-20.91	30.00
New York	-10.89	15.71	-4.86	16.13	-28.57[a]	28.57[a]	n.a.	n.a.
Philadelphia	-4.17	6.67	2.43	2.70	-5.41	5.41	3.65	4.35
Total	-4.68	8.43	-3.58	13.97	1.14	11.94	-11.72	21.05

NOTE: These data are based on simple subgroup means. No test statistics were computed.

[a] Sample size is less than 20.

n.a. = not applicable.

TABLE A.19

ESTIMATED PROGRAM IMPACTS ON HOURS WORKED PER MONTH, UNDER OBSERVED PATTERN OF UC RECEIPT AND ASSUMING NO RECEIPT AMONG EXPERIMENTALS

	Months 10-18		Months 19-27	
	Observed UC Receipt	No UC Receipt	Observed UC Receipt	No UC Receipt
All Youth	11.7	13.2	0.6	1.8
Site				
Atlanta	5.5	7.4	-15.5	-9.4
Hartford	11.9	12.0	3.9	3.9
Jersey City	-0.2	4.3	-11.2	2.8
New York	22.5	22.7	21.2	21.2
Philadelphia	30.7	30.7	14.3	14.3

NOTE: For sample definitions, see Table II.2. Very few experimentals received unemployment compensation during the first nine months after enrolling in the demonstration and during the 28- to 36-month period after enrollment. Therefore, no adjusted figures are presented for either of these time periods. The estimates assuming no UC receipt are based on a comparison of hours worked by experimentals who did not receive UC and by controls, since we found no evidence that there was any significant behavioral difference between the UC recipients and nonrecipients, based on either their 1- to 9- or their 28- to 36-month employment.

Significance levels for the above differentials were not calculated.

TABLE A.20

PERCENTAGE DISTRIBUTION OF INDUSTRIES OF FIRST NON-PROGRAM JOB

YOUTH SAMPLE

	Sample with 18 Months of Follow-Up Data		Sample with 27 Months of Follow-Up Data		Sample with 36 Months of Follow-Up Data	
	Experimental Group	Control Group	Experimental Group	Control Group	Experimental Group	Control Group
Agriculture, Forestry, or Fishing	3.6	3.7	3.3	3.5	2.2	0.0
Mining	0.0	0.0	0.0	0.9	0.0	0.0
Construction	8.2	9.2	7.7	6.2	6.7	6.1
Manufacturing	30.9	25.0	26.4	26.6	26.7	53.1
Transportation	10.0	8.5	11.0	4.4	13.3	8.2
Wholesale Trade	0.9	1.8	4.4	1.8	0.0	2.0
Retail Trade	20.9	24.4	17.6	27.4	20.0	14.3
Finance, Insurance, and Real Estate	1.8	3.1	3.3	6.2	2.2	0.0
Services	23.6	24.4	26.4	23.0	28.9	16.3
(Percent with Non-Program Employment)	(65.8)	(78.4)	(74.8)	(82.8)	(90.3)	(93.2)
Number in Sample	110	164	91	113	45	49

NOTE: Samples are defined as noted in Table III.9. The data are not regression-adjusted.

TABLE A.21

PERCENTAGE DISTRIBUTION OF OCCUPATIONS OF FIRST NON-PROGRAM JOB

YOUTH SAMPLE

	Sample with 18 Months of Follow-Up Data		Sample with 27 Months of Follow-Up Data		Sample with 36 Months of Follow-Up Data	
	Experimental Group	Control Group	Experimental Group	Control Group	Experimental Group	Control Group
Professional, Technical, or Managerial	3.4	4.7	2.0	0.8	3.9	1.9
Clerical or Sales	16.1	19.2	10.0	18.5	13.7	18.9
Service	22.0	25.6	29.0	31.1	29.4	17.0
Agriculture, Fishing, Forestry, or Related	3.4	2.3	3.0	4.2	3.9	0.0
Processing	0.0	4.1	1.0	1.7	3.9	1.9
Machine Trades	8.5	8.7	10.0	5.9	5.9	13.2
Benchwork	10.2	4.7	7.0	5.9	2.0	3.8
Structural Work	9.3	14.5	15.0	10.1	5.9	7.6
Miscellaneous	27.1	16.3	23.0	21.9	31.4	35.9
(Percent with Non-Program Employment)	(65.8)	(78.4)	(74.8)	(82.8)	(90.3)	(93.2)
Number in sample	118	172	100	119	51	53

NOTE: Samples are defined as noted in Table III.9. The data are not regression adjusted.

TABLE A.22

PERCENTAGE LEAVING FIRST NON-SUPPORTED WORK JOB FOR VARIOUS REASONS

YOUTH SAMPLE

	Experimentals	Controls
Had a Better Opportunity (job or school)	9.6	6.2
Job Characteristics (low pay, did not like type of work)	39.0	30.6
Personal Reasons (health, drugs)	11.9	13.9
Problems Obtaining Childcare	n.a.	n.a.
Transportation	2.5	2.6
Poor Performance	5.3	7.0
Lack of Work	50.4	35.6
Did Not Want to Work	8.1	1.5
Other	14.6	11.6
Number in Sample	267	271

NOTE: The sample includes all sample youth who completed at least an enrollment, a 9-month, and an 18-month interview and who had held at least one non-program job which they left during the follow-up period.

TABLE A.23

LABOR FORCE STATUS AND JOB SEARCH DURING THE LAST FOUR WEEKS IN RESPECTIVE TIME PERIODS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Labor Force Status (percent)								
In labor force	9.42**	71.66	-3.36	76.35	-4.44	75.75	5.25	46.75
Employed	28.17**	42.44	1.53	52.80	2.52	56.65	-2.56	100.00
Unemployed	-28.17**	57.56	-1.53	47.20	-2.52	43.35	2.56	0.0
Not in labor force	-9.42	28.34	3.36	23.65	4.44	24.25	-5.25	53.25
Job Search Activity								
Percentage looking for work	-11.35**	57.43	-0.32	51.10	5.92	47.93	5.88	42.65
With help of								
Supported Work	0.79	0.87	1.04	0.00	0.00	0.00	0.00	0.00
State employment agency	-11.55**	28.02	-5.76	34.93	-4.35	47.83	-14.95	48.28
CETA	-3.80**	4.35	-0.70	3.83	3.53	5.17	-13.79**	13.79
Community Organization	0.52	3.90	-0.57	6.22	-2.18	8.70	-3.87	6.90
Private agency	2.56	1.29	-1.01	6.22	2.67	6.03	-0.42	3.45
Other	3.88	87.83	3.15	88.52	3.15	87.07	-4.39	86.21
Average number of contacts	0.05	2.45	-0.23	2.82	-0.65	2.66	-0.16	2.09
Hours per week spent looking for work	-1.44**	4.46	0.30	3.76	-0.45	3.65	-1.24	3.82
Reservation wage (dollars per week)	14.90**	107.81	5.02*	124.91	10.99**	131.42	10.42	138.44
Among those employed	15.98**	111.24	5.05	129.25	-1.51	144.65	12.13	146.00
Among those unemployed	9.57**	104.20	6.11	119.84	14.65**	120.17	a/	a/
Among those not in the labor force	11.48	109.73	3.47	124.87	26.55**	123.50	10.04	131.46

NOTE: These data are based on simple subgroup means. Samples used are defined in Table II.2.

a/ There were no control-group members among the unemployed.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.24a

PERCENTAGE RECEIVING IN-KIND ASSISTANCE

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Medicaid	5.8**	22.0	-8.5**	24.7	-3.3	22.2	-5.0	21.0
Public Housing	5.8*	32.0	-5.1	30.9	-6.7	30.7	-5.7	27.1
Rent Subsidy[a]	0.1	0.9	0.1	1.4	-1.5	2.5	-3.3	3.3

TABLE A.24b

AMOUNT OF IN-KIND ASSISTANCE RECEIVED

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Number of Months with Medicaid Card	-0.6**	1.5	-0.4	1.6	-0.3	1.7	-0.6	1.7
Number of Subsidized Doctor Visits	-0.2	0.6	0.1	0.5	0.1	0.2	-0.1	0.3
Number of Subsidized Hospital Days	-0.2	0.7	-0.2	0.4	0.2	0.6	0.0	0.2
Monthly Public Housing Rent (dollars)[b]	11.93**	109.64	5.48	118.02	2.15	120.31	18.52	101.38
Monthly Rent Subsidy (dollars)	0.58	1.04	0.31	1.66	-1.18	2.92	-5.70	5.70

NOTE: The samples used are defined in Table II.2. These data are simple subgroup means, except those pertaining to subsidized doctor visits and hospital stays, which are regression-adjusted. All figures pertain to the full sample, not only to recipients.

[a] A rent subsidy is defined as rent paid directly to the landlord by the welfare agency.

[b] These figures apply only to public housing residents.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.25

PERCENTAGE REPORTING ANY ARREST
BY CURRENT EMPLOYMENT STATUS

YOUTH SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Not Employed	-7.4	19.9	4.0	19.1	3.8	10.4	-19.0	44.0
Employed	-0.0	17.0	-0.4	14.0	-8.3**	17.2	17.4**	4.0
(Percentage Employed)[a]	(45.0)**	(53.0)	(8.5)**	(59.9)	(1.0)	(60.8)	(7.5)	(66.2)

NOTE: For definitions of the samples used, see Table II.2. These data are not regression-adjusted.

[a] These data may differ somewhat from those reported in Chapter III because of the slight differences in the samples used and because these data are not regression-adjusted.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.26

TOBIT ESTIMATES OF NUMBER OF ARRESTS

YOUTH SAMPLE

	Months 1 - 18		Months 1 - 27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Youth	.0148	.3170	-.2012	.5608
Site				
Atlanta	-.0249	.1630	-.5155[a]	.5155[a]
Hartford	.0733	.3423	.0198	.6684
Jersey City	-.1151	.3301	-.2508*	.5199
New York	.1427	.3527	.5620[a]	.1469[a]
Philadelphia	-.0538	.3165	-.1745	.5422
Number in Sample	809		379	
Mean Number of Arrests	.434		.660	

NOTE: Samples used are defined in Table II.2.

[a] The sample size is less than 20. Thus, these point estimates of experimental-control differentials tend to be quite unreliable.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

APPENDIX B

ASSESSING THE IMPACT OF INTERVIEW NONRESPONSE ON EVALUATION RESULTS

By Randall Brown*

* This appendix is excerpted from a project report "Assessing the Effects of Interview Nonresponse on Estimates of the Impact of Supported Work," Princeton, New Jersey: Mathematica Policy Research, Inc., 1979.

I. THE PROBLEM

The primary methodology used in the analysis of the effects of the Supported Work program is the single equation multiple regression model. In the simplest case, outcomes of interest (such as earnings, employment, and drug use) are regressed on personal characteristics and on a dummy variable equal to one for experimentals and zero for controls. The coefficient on the experimental status variable reflects the difference between experimentals and controls, while the other explanatory variables account for differences in the other characteristics, so that the comparison of experimental and control groups is for groups with similar composition. Alternative specifications include interaction terms between the experimental-control dummy and personal characteristics, in the belief that the program's impact may depend upon the socioeconomic characteristics of the participant. Interactions of the experimental status variables with location or length of site operation may also be included in the model as explanatory variables. The general regression model can be written as

$$Y = XB + \varepsilon, \qquad (1)$$

where Y is the outcome variable, X is a matrix containing demographic and socioeconomic characteristics as well as program variables, ε is a disturbance term, and B is a vector of unknown parameters to be estimated.

Estimation of B is usually accomplished by the use of ordinary least squares (OLS) regression methods, where the OLS estimator can be written as:

$$\hat{B} = (X'X)^{-1} X'Y. \qquad (2)$$

Substituting (1) into (2) we have

$$\hat{B} = B + (X'X)^{-1} X'\varepsilon. \qquad (3)$$

For a sample in which no systematic effect is operating to limit the sample available for analysis--that is, an uncensored sample--the expected value of the regression coefficient is

$$E\ (\hat{B}|X) = B + (X'X)^{-1}\ X'E\ (\varepsilon|X). \qquad (4)$$

Thus, $\hat{B}$ is an unbiased estimator of B if $E\ (\varepsilon|X) = 0$; that is, if the conditional mean of the disturbance term is equal to zero. This condition is usually assumed to be satisfied for a properly specified model.

For a censored sample, however, we have the additional conditioning factor of the sample selection rule. Hence,

$$E\ (\hat{B}|X \text{ and selection rule}) = B + (X'X)^{-1}\ X'E\ (\varepsilon|X \text{ and selection rule}) \qquad (5)$$

If the conditional expectation of the disturbance term fails to equal zero, the coefficients will be biased. Thus, attention must focus on the relationship between the sample selection rule and the disturbance term ε.

The censoring mechanism in the case under consideration here is failure to obtain a scheduled follow-up interview (for any reason) for an individual. One way to view this mechanism is to imagine that each individual has an index of response likelihood, R^*. Individuals with values of R^* exceeding zero will be locatable and will be able and willing to complete the interview. Those with values of R^* below zero will not complete interviews. Furthermore, assume that it is possible to identify some characteristics that affect the likelihood of response, such as whether the individual has moved, whether he or she is incarcerated, and a variety of other personal traits. This model can be described as follows:

$$R^* = Z\delta + \eta, \qquad (6)$$

where Z is a vector of personal traits affecting responsiveness,[1] δ is the coefficient vector, and η is a disturbance term. Of course, R* is not observed directly; we only know whether or not an interview was completed:

$$R = \begin{cases} 1 \text{ for } R^* \geq 0, \text{ (i.e., } \eta \geq -Z\delta) \\ 0 \text{ for } R^* < 0, \text{ (i.e., } \eta < -Z\delta) \end{cases}, \qquad (7)$$

where R = 1 for respondents and R = 0 for non-responders.

From equation (5) it can be seen that in order to obtain unbiased coefficients we require

$$E\ (\varepsilon | X, \eta \geq -Z\delta) = 0. \qquad (8)$$

If ε has zero mean and ε and η are mean independent,[2] this condition is satisfied (for nonstochastic Z). However, if the probability of nonresponse is affected by Y (and therefore by ε), ε and η are not independent, the expected value in equation (8) is not zero, and the regression estimates of the coefficients in equation (1) will be biased.[3]

---

[1] The vector Z may contain many of the same variables as X contains.

[2] Mean or conditional independence implies that $E\ (\varepsilon|\eta) = E\ (\varepsilon)$, a somewhat stronger requirement than zero correlation, unless ε and η are assumed to have a bivariate normal distribution.

[3] As noted by Peck (1973) and others, if the probability of nonresponse is related only to the regressors (X's) or is random, no nonresponse bias results.

This correlation between ε and η may result in two different ways. If Z contains only nonstochastic variables, and there exists an unmeasured variable (e.g., motivation or attitude) that affects both outcomes and the probability of response, then ε and η will be correlated. However, correlation of the disturbance terms of the estimating equations will result even if the disturbance terms in the structural equations are independent if current outcomes affect the probability of responding to requests for interviews. In this case the structural response model can be written as

$$R^* = X\delta_1 + Z^*\delta_2 + Y\delta_3 + \eta^*, \qquad (9)$$

where Z^* contains exogenous variables not included in X, and η^* is a disturbance term possibly uncorrelated with ε.[1] Substituting equation (1) in (9) to obtain an equation that can be easily estimated we have

$$\begin{aligned} R^* &= X\delta_1 + Z^*\delta_2 + (X\beta + \varepsilon)\delta_3 + \eta^* \qquad (10) \\ &= X(\delta_1 + \beta\delta_3) + Z^*\delta_2 + (\eta^* + \varepsilon\delta_3), \\ R^* &= Z\delta + \eta, \qquad (11) \end{aligned}$$

where $Z = (X, Z^*)$, $\delta = \begin{pmatrix} \delta_1 + \beta\delta_3 \\ \delta_2 \end{pmatrix}$, and $\eta = (\eta^* + \varepsilon\delta_3)$.

Clearly, the disturbance term in the estimating equation (11), which has the same form as equation (6), is correlated with ε, even if the disturbance terms η^* and ε are independent.[2]

---

[1] Some of the elements of δ_1 will be zero if there are variables in X which affect outcomes but not response.

[2] The only difference between the two behavioral specifications that affects estimation of the model of probability of response is that equations (9)-(11) result in the inclusion of all exogenous variables from the outcome equation (1), including ones not considered to have direct impact on the likelihood of response. Only variables directly affecting response are included in the vector labeled Z under the first specification, (6).

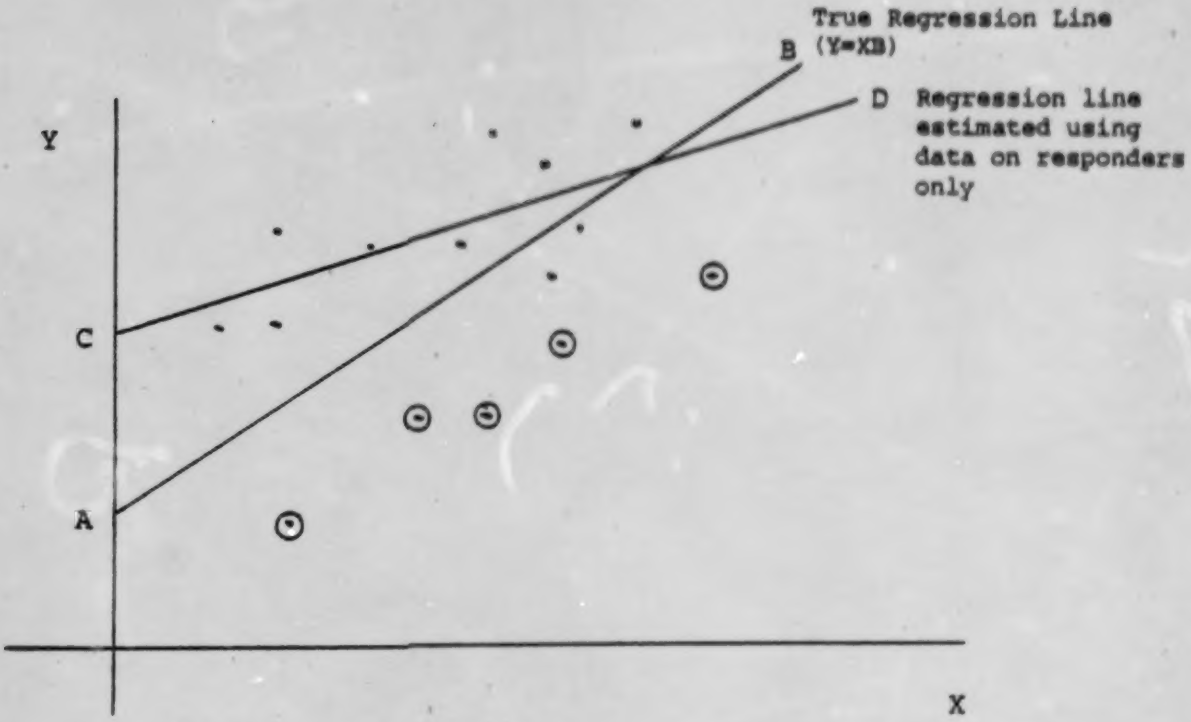
Figure B.1 provides an intuitive explanation of the problem. For a given vector Z, individuals with large negative values of η are more likely to be nonresponders. If η and ϵ are positively correlated, the nonresponders are more likely to be those with large negative deviations (ϵ) from the true regression line, AB--that is, those corresponding to the circled points in the diagram. Performing regression analysis on the restricted sample would produce an estimated regression line like CD. Comparison of CD with the true regression line AB demonstrates the potential for bias in estimated coefficients arising from nonresponse.

Recent developments in econometric methodology suggest ways of handling the problem of nonresponse bias when data on the variables affecting the probability of response (Z) are observed. Heckman (1976) shows that statistical models characterized by limited dependent variables, sample selection rules, or truncation points have a common structure, and suggests a simple method of estimating these models that we employ in this analysis.

Heckman's paper deals with the estimation of models like those specified in equations (1), (6), and (7). In an earlier paper (Heckman, 1974), he showed that maximum likelihood methods could be employed to consistently and efficiently estimate the parameters of this model. However, the likelihood method was found to be quite expensive. The more recent paper (Heckman, 1976) shows that consistent estimates can be obtained in a much less costly manner by treating the problem as an "omitted variable" problem. Using our nonresponse model to

FIGURE B.1

ILLUSTRATED EFFECT OF SAMPLE NONRESPONSE ON ESTIMATES OF REGRESSION PARAMETERS



Note: Circled observations are those omitted from the evaluation sample because of nonresponse.

242

demonstrate, this can be seen as follows: equation (1) for the i^{th} observation is

$$Y_i = X_i B + \varepsilon_i. \qquad (12)$$

Taking expectations, given that the sample available is limited to those who respond ($R^* \geq 0$), gives

$$E\,(Y_i | R_i^* \geq 0) = X_i B + E\,(\varepsilon_i | R_i^* \geq 0). \qquad (13)$$

If we assume that ε and η, the disturbance term in equation (6), follow a bivariate normal distribution, then it can be shown[1] that

$$E\,(\varepsilon_i | R_i^* \geq 0) = \frac{\sigma_{12}}{(\sigma_{22})^{\frac{1}{2}}}\,\lambda_i, \qquad (14)$$

where σ_{12} is the covariance between ε and η, σ_{22} is the variance of η, and

$$\lambda_i = \frac{f\,(Z_i\delta/\sigma_{22}{}^{\frac{1}{2}})}{F\,(Z_i\delta/\sigma_{22}{}^{\frac{1}{2}})}. \qquad (15)$$

The denominator of λ_i is the probability that $R_i^* \geq 0$ (i.e., the probability that the individual responds to the interview), while the numerator of λ_i is the standard normal density function, evaluated at the point $Z_i\delta/\sigma_{22}{}^{\frac{1}{2}}$.

Substituting equation (14) in equation (13) we have

$$E\,(Y_i | R_i^* \geq 0) = X_i B + \frac{\sigma_{12}}{(\sigma_{22})^{\frac{1}{2}}}\,\lambda_i. \qquad (16)$$

Estimation of equation (12) on the sample of respondents will not take into account the final term in equation (16). Thus, the bias that arises from use of this "censored" sample exists solely because the conditional mean of ε_i is omitted from the regression. The bias that results from use of respondent-only data may then be interpreted as arising from normal specification error. This interpretation suggests a simple solution: provide an instrument for the missing variable (λ_i) and estimate equation (16). Heckman (1976) proposes just this solution to the general problem of selection bias. His approach (applied to our model) suggests that if data on the variables (Z) determining the likelihood of response are available, an approximation to λ_i can be obtained by estimating a probit model of response, such as that implied by equations (6) and (7), and then using the estimated coefficients to form $\hat{\lambda}$ for each observation. Equation (16) can then be readily estimated by ordinary least squares regression. Although the equation still must be fit only on data from respondents, any bias that this might impart to the coefficients, $\hat{B}$, is corrected for by inclusion of the λ_i term,[1] if the assumptions of the model hold and λ is reliably estimated.

---

[1] See Johnson and Kotz (1972), pp. 112-116.

For this study, we are interested only in bias in the coefficient measuring experimental impact. Adding λ to the estimating equation will change our estimate of Supported Work's impact only to the extent that λ is correlated with status. Hence, we shall be particularly concerned with those cases in which experimental status affects the probability of response.

---

[1] The estimates of B are unbiased only asymptotically, since an estimate of λ_i must be substituted for the unobserved true value in the regression.

In the next section, a model to explain response to Supported Work interviews is developed, and the results from this estimation are used in the final section to implement Heckman's approach to correct for selection bias.

II. A MODEL OF THE PROBABILITY OF RESPONSE TO SCHEDULED INTERVIEWS

The probability that an individual will respond is assumed to depend upon his or her demographic characteristics, past and present behavior, and experience with the enrollment interview. While this includes many of the same variables that are important control variables in the outcome regressions, equations (9)-(11) suggest that all variables affecting outcomes should be included in the response model, even if they are felt to have no direct impact on response responsibilities. In addition, a number of variables that are assumed to have no impact on outcomes but that are felt to affect the probability of response are included in the model. Examples of such variables are the number of moves made during the two years prior to enrollment (since those moving are often the hardest to locate); some variables describing personal living arrangements; expected earnings if employed; whether the individual applied to Supported Work because of some agency pressure to find a job; some indicators of the nature of the interviewing process itself, such as the length and location of the baseline interview; and the individual's degree of cooperativeness in completing the enrollment interview (as reported by the interviewer).

Because the data on these determinants of response are collected from the enrollment interview, the parameters of the model of response to the follow-up interviews can be estimated. From equations (6) and (7), assuming η has a standard normal distribution, we have

$$\begin{aligned} P\ (R_i = 1) &= P\ (R_i^* \geq 0) \\ &= P\ (Z_i\delta + \eta_i \geq 0) \\ &= P\ (\eta_i \leq Z_i\delta) \\ &= \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{Z_i\delta} \exp\ (-\eta_i^2/2)\ d\eta_i . \end{aligned}$$

Forming the likelihood function for the sample gives

$$L = \Pi_i\ [P\ (R_i = 1)]^{R_i}\ [1 - P\ (R_i = 1)]^{1-R_i}.$$

Estimates for the parameters of this probit model, δ, are those values that maximize L, and are readily obtained from a probit computer program.

Sample sizes used for this analysis and for the subsequent regressions to estimate the impact of nonresponse on the evaluation results are shown in Table B.1 for each time period and target group. The number of observations available for the 36-month analysis is quite small and therefore may be of limited value, but for other time periods sample sizes are generally adequate.

Response equations were estimated for each time period. The results, presented in Table B.2, show that sex, race, living arrangements, and length of longest job were important determinants of nonresponse for the early interviews and the 27-month interview. Females (who made up 10 to 13 percent of the sample) were more likely than males to respond.

TABLE B.1

SAMPLE SIZES USED IN ESTIMATING RESPONSE MODEL AND IN REGRESSION ANALYSIS

(Response Rates in Parentheses)

	Months 1-9	Months 10-18	Months 19-27	Months 23-39
Nonresponse Model	[a]	1,080 (69)	602 (71)	131 (79)
Respondents		750	430	130
Nonrespondents		330	172	28
Outcome Regressions[b]	709	687	390	90

NOTES: The numbers of respondents differ from the sample sizes used in the regression model because of observations with missing data on the specific dependent variables examined. Although these observations are also lost to analysis and thus could be considered nonresponders, it is unlikely that the same model applies to both interview nonresponse and item nonresponse. Because missing data items could result from several causes (including coding errors) and because the number of respondents with missing values for the desired dependent variables is generally small, item nonresponse is ignored here.

The response rates presented here differ slightly from those given in Table A.3 because observations with missing values on necessary baseline explanatory variables were excluded from this analysis.

[a] Individuals were classified as responders in the 1-9-month and 10-18-month analyses only if they completed both interviews. Hence, the sample sizes given in the column headed "Months 10-18" apply here as well.

[b] These sample sizes differ from those in the main body of this report because observations with missing data on needed baseline variables were excluded from this analysis, but not from the main analysis.

TABLE B.2

THE IMPACT OF PREENROLLMENT CHARACTERISTICS AND PROGRAM VARIABLES ON THE PROBABILITY OF RESPONSE TO THE FOLLOW-UP INTERVIEWS

Variable	Follow-Up Interview		
	9- and 18-Month	27-Month	36-Month
VARIABLES ALSO USED IN OUTCOME ANALYSIS REGRESSIONS			
Member of experimental group	8.13***	3.47	-7.25
Site[a]			
Atlanta	1.61	-11.59	n.a.
Hartford	-4.69	-11.04	-35.47
Jersey City			
New York	10.53	-32.51**	n.a.
Philadelphia	-2.80	4.36	-9.34
Education			
(Less than 9 years)			
≥ 9 years	-3.23	2.71	9.84
Age			
(Under 19)			
≥ 19	.20	-4.03	15.64**
Male	-8.72*	-18.71**	20.91*
Race			
(Black and other)			
White	-26.42***	-18.03***	-29.32***
Hispanic	-9.84**	-5.23	-15.64*
• Household size	.32	.87	3.60**
Any welfare prior year	3.39	.53	13.03
Any dependents	6.77	1.10	-31.58**
Technically eligible for own target group	-3.63	-5.13	-12.91*
Length of site operation			
(Under 12 months)			
12-18 months	4.60	-6.93	n.a.
Over 18 months	-1.65	1.89	n.a.
Longest job ever			
(None)			
1 year or less	6.94*	-8.94*	-8.62
Over 1 year	8.10	-9.35	-17.82
• Weeks worked last year	-.05	.27	-.10
• Area unemployment rate	1.77	.21	-4.21
Time since last incarceration			
(Never incarcerated)			
12 months ago or less	-4.80	-7.31	1.41
More than 12 months ago	-4.27	3.97	25.80**
Had any arrest last year	.43	10.32**	-8.36
• Number of arrests (ever)	-.06	.14	-1.68**
Ever use any drug (except marijuana or alcohol)	-2.92	5.96	15.59
VARIABLES NOT USED IN OUTCOME ANALYSIS REGRESSIONS			
Residence (Institution)			
Own Home	27.85**	27.14**	-6.24
Other's home	16.53*	19.27	n.a.
Rent	14.32*	17.57	-.44

TABLE B.2 (CONT'D)

Variable	Follow-Up Interview		
	9- and 18-Month	27-Month	36-Month
Baseline interview in SW office	-6.80	4.27	3.13
• No. of moves in last 2 years	.05	2.13	1.36
• Expected wage per week ($100)	.85	.50	-10.71**
Pressured to find job	.77	2.62	-13.44
Live in public housing	7.15**	.10	-5.18
Live with parents	12.33***	12.48**	-18.25
• Length of interview (minutes)	-.05	-.07	-.23
Respondent cooperative	7.91	5.22	-8.70

NOTES: The effect of a change in a continuous variable Z_1 on the probability of response is $\delta_i * f(Z\delta)$, where δ is the coefficient on Z_i in equation (11) (the probit model) and $f(Z\delta)$ is the standard normal density function, evaluated at the point $Z\delta$. This expression also serves, in most cases, as a very good approximation to the effect of a change in a discrete (dummy) variable on the probability of response. Hence, this is the method used to compute the marginal impacts presented here. The density function $f(Z\delta)$ is evaluated using the mean values for all the variables in Z. All of these partial impacts are expressed in terms of percentage points $(100 * \delta_i f(Z\delta))$.

For continuous variables (those marked with • in the left-hand margin), a change of one unit is predicted to lead to a change in the probability of responding equal to the value given, all other factors being equal. For discrete variables (those not marked with •), there may be two or more possible values. Race, for example, has three possible values (black, Hispanic, or white), while "live with parents" has only two possible values (yes or no). For variables with only two possible values, the value given in the table is the difference in the probability of response for those who do and do not exhibit the given trait. For variables with three or more outcomes, the value given is the amount by which the predicted probability of response for individuals with the specified characteristics exceeds the expected response probability for those with the characteristics given in parentheses.

a/ Jersey City is the excluded site.

*Estimate of coefficient corresponding to this variable is statistically significant at the 10 percent level (two-tailed test).

**Estimate of coefficient corresponding to this variable is statistically significant at the 5 percent level (two-tailed [illegible]).

***Estimate of coefficient corresponding to this variable is statistically significant at the 1 percent level (two-tailed test).

• Indicates variable is continuous. All others are discrete.

n.a. means not applicable.

Among ethnic groups, there were substantial and significant differences in the probabilities of response, with whites being the least likely to respond, followed by Hispanics and then blacks. Youth living with their parents were also much more likely to respond to the 9-, 18-, and 27-month interviews than those who did not. Also, those living in institutions were less likely to respond than those living elsewhere, and renters tended to be somewhat less likely to respond than those living in their own homes.

The findings for length of longest job were perverse. Those who had held a job for up to one year were more likely to respond to the 9- and 18-month interviews than those who had never had a job, but were significantly less likely than nonworkers to respond to the 27-month interview.

A few other variables were found to be statistically significant determinants for either the early or the 27-month interviews--but not both--including whether lived in public housing, whether arrested, and experimental status. This latter finding is the most significant for this analysis. The results indicate that experimentals were significantly (8 percentage points) more likely to respond to the 9- and 18-month interviews than controls. This factor suggests that we should be particularly concerned about potential nonresponse bias in results for the 10- to 18-month period.

Results for the 36-month sample were quite different than for the 9-, 18-, and 27-month samples, due primarily to the small sample size (N=131). The relationship between race and probability of response found for the earlier interviews was also apparent for the 36-month interviews, but this was the only common finding. Several variables had statistically significant coefficients in the response equation, but in many cases the direction of the estimated relationship was counterintuitive. Furthermore, a χ^2 test of the hypothesis that all the coefficients were equal to zero could not be rejected at the one percent level (although this hypothesis was rejected at the five percent level). Thus, the results for the 36-month sample should be viewed with considerable suspicion.

III. THE EFFECT OF NONRESPONSE ON ESTIMATED PROGRAM IMPACTS

With the estimates of the parameters of the nonresponse model, we can construct the estimate of that part of the disturbance term in equation (12) that is correlated with the regressors Z. As explained previously, this procedure yields a new variable, $\hat{\lambda}$, which can then be included as an additional regressor in the estimation of equation (12). Under the assumptions of the procedure, this regression produces asymptotically unbiased estimates of the effect of experimental status (and control variables) on the outcome variable (Y) of interest, despite the fact that only data on responders is used in the regression. Comparison of these results with the estimates obtained with $\hat{\lambda}$ excluded provides evidence of whether or not analysis of data on responders leads to unbiased inferences about the impact of Supported Work.[1/]

[1/] As pointed out previously, the reliability of this evidence depends upon the validity of the assumptions involved in the model. Furthermore, although discrepancies between the alternative estimates suggest that there is likely to be nonresponse bias, a correspondence of the two sets of estimates may indicate only that the model of nonresponse is not good enough to permit detection of bias.

Although unadjusted estimates of program effects are presented in the main body of this report, we repeated the calculations on the sample analyzed here in order to ensure that any differences between the adjusted and unadjusted estimates of program impact result from the adjustment alone rather than to differences in the samples used.[1]

Although many different outcome variables are examined in the final reports on the effects of Supported Work, only a subset of the more important outcomes has been selected for examination here. These are hours worked, earnings, whether participants were arrested, and whether drugs were used for each of the four nine-month periods.[2]

Estimates of the impact of Supported Work on each of these outcomes, both with and without correction for possible nonresponse bias, are contained in Table B.3. In general, the alternative sets of estimates are very similar. Estimates that were statistically insignificant prior to adjustment for potential bias remained insignificant, while

---

[1] The methodology employed treats as nonresponders only those who did not submit to an interview. However, observations were also made unavailable for analysis when respondents failed to answer specific key questions. For a number of reasons, including the fact that only a small number of observations was involved, we ignored this type of nonresponse. Also, observations with insufficient data on personal characteristics were excluded from the analysis. These were often respondents who had received early versions of the enrollment interview.

[2] It should be pointed out that each of these variables is either a binary variable (such as whether arrested) or a limited dependent variable (hours worked). Hence, ordinary least squares regression is not the most appropriate method of analysis. However, for cost reasons it is the primary methodology used throughout the analysis of the effects of Supported Work. Since the purpose of this Appendix is to determine whether the results of these analyses suffer from nonresponse bias, we employ the same estimation techniques. It should also be noted that comparison of the least squares results to those obtained from more appropriate techniques such as probit and tobit showed very little difference.

TABLE B.3

REGRESSION-ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS FOR SELECTED VARIABLES, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
Outcome Measure	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Hours worked per month	83.11***	85.36***	13.21**	15.88***	6.61	8.77	-18.72	-7.58
Earnings per month (dollars)	217.69***	226.54***	28.19	28.36	21.94	31.32	-166.70**	-124.85*
Probability of arrest (x100)	1.40	.24	2.24	.45	-3.41	-3.90	1.53	-2.82
Probability of drug use (x100)	-1.86	-1.45	-.18	-.41	.48	.82	-4.53	-3.39

NOTE: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice, the true test statistics are usually very close to the ones reported by the regression program. Hence, the significance levels given here are indicative of the actual significance levels.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

n.a. means not applicable.

those that were significant exhibited almost no change in size. The two exceptions to this were for hours worked during the 10- to 18-month and the 28- to 36-month periods. Prior to adjustment for bias, the estimated experimental effect for the earlier period was 13.2 hours per month. After adjustment for potential bias, this coefficient increased to 15.9, an increase of 20 percent. This finding is consistent with the results from the probit models of nonresponse in which we found that status had a significant impact on response rates.[1]

For the 28- to 36-month period, the adjusted and unadjusted results differ more widely: the estimate of the earnings impact decreased by 34 percent from $167 per month to $125 per month. Because the 28- to 36-month sample size is so small (90 observations), this result is likely to be a statistical anomaly not worthy of much attention.

In general, statistically significant differences did not change after correcting for nonresponse, nor did the general order of magnitudes of estimates of program impacts. Thus, these findings suggest that nonresponse bias does not seem to be a prevailing problem for the analysis, at least when the effects of status are modeled in the simple way used here. It is also important to determine, however, whether estimates of Supported Work's impact are biased when such estimates are allowed to vary with characteristics of the program. One finding that occurs regularly is that program impacts differ by site. Hence, we also examined estimates of program impact obtained from a model that takes this into account for evidence of nonresponse bias.[1]

---

[1] An intuitive explanation for this finding for youth hours, months 10 to 18, is as follows. The regression coefficient on λ in the "adjusted" equation for youth hours was positive and significant. From equation (16), we see that this coefficient is an estimate of $\sigma_{12}/(\sigma_{22})1/2$. Hence, the covariance of the disturbance terms in the hours and response equations is positive. This suggests that those with higher hours worked are more likely to be responders, other things being equal, and those working less are likely to be nonresponders. From the nonresponse model results, we know that in the youth group, controls were significantly less likely to respond to the 9- and 18-month interviews than were experimentals. Thus, the observations excluded from the analysis because of nonresponse were more likely to be nonworking controls. Inclusion of such observations would increase the estimated experimental-control difference. This is precisely what the methodology employed does.

The results contained in Tables B.4 to B.7 show little evidence of substantial nonresponse bias for the 10- to 18-month and 19- to 27-month periods. Estimates of program effect on hours and earnings for the various sites change somewhat after adjustment. However, most of these estimates are small and statistically insignificant, both before and after adjustment. Of those that are significant, the largest change is for Hartford (months 10-18), where the estimated experimental effect increases from 14 to 18 hours per month (an increase of 33 percent).

We find no evidence of bias for the other outcome measures, however, for any time period. The sample sizes for the 28- to 36-month period are so small that meaningful inferences about program impact for the various sites cannot be drawn. Hence, the issue of bias for this period is moot.

---

[1] Estimates were also obtained for models in which estimates of program impact were allowed to vary with the length of site operation. Since no evidence was found to suggest that this program characteristic had a systematic effect on program impact, these results are not presented here.

TABLE B.4

HOURS WORKED PER MONTH, REGRESSION ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS

YOUTH SAMPLE

	Months 1-9		Months 10-18		Months 19-[illegible]7		Months 28-36	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Site								
Atlanta	82.23***	84.83***	10.37	13.18	-45.82	-38.35	n.a.	n.a.
Hartford	89.70***	93.63***	13.78*	18.37**	8.32	10.05	-41.27	2.55
Jersey City	97.67***	98.37***	5.30	5.91	5.06	8.03	-24.31	-17.66
New York	62.30***	63.74***	22.83*	24.17*	18.14	19.82	n.a.	n.a.
Philadelphia	35.08*	36.16**	13.20	12.69	7.98	8.04	9.12	9.74

Note: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice the true test statistics are usually very close to the ones reported by the regression program. Hence the significance levels given here are indicative of the actual significance levels.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

n.a. means not applicable.

TABLE B.5

DOLLAR EARNINGS PER MONTH, REGRESSION ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS

YOUTH SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Site								
Atlanta	178.71***	188.38***	-4.77	-3.78	-293.75	-236.69	n.a.	n.a.
Hartford	233.79***	248.42***	39.79	41.41	51.20	58.16	-222.20	-51.59
Jersey City	251.56***	254.17***	- .59	- .38	-10.37	1.59	-194.41**	-168.52*
New York	195.90***	201.27***	59.79	60.26	32.71	39.47	n.a.	n.a.
Philadelphia	75.78	79.79	-11.50	-11.68	28.79	29.02	-66.07	-63.65

Note: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice the true test statistics are usually very close to the ones reported by the regression program. Hence the significance levels given here are indicative of the actual significance levels.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

n.a. means not applicable.

TABLE B.6

PROBABILITY OF BEING ARRESTED (X100), REGRESSION ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS

YOUTH SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Site								
Atlanta	8.67	7.58	3.06	1.26	5.76	4.05	n.a.	n.a.
Hartford	1.66	.01	1.95	- .99	-6.36	-6.76	.91	-18.73
Jersey City	-5.88	-6.18	3.71	3.32	-2.52	-3.21	1.26	-1.72
New York	4.84	4.23	6.18	5.32	26.68	26.29	n.a.	n.a.
Philadelphia	2.16	1.71	-11.86	-11.53	-2.95	-2.96	2.55	2.27

Note: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice the true test statistics are usually very close to the ones reported by the regression program. Hence the significance levels given here are indicative of the actual significance levels.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

n.a. means not applicable

TABLE B.7

PROBABILITY OF USING DRUGS, REGRESSION ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS

YOUTH SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Site								
Atlanta	-5.25	-4.78	1.98	1.83	.30	1.00	n.a.	n.a.
Hartford	-1.13	- .42	1.94	1.74	2.40	2.56	-24.83	-24.07
Jersey City	-5.17	-5.05	-8.78*	-8.82*	-6.50	-6.23	-1.19	-1.07
New York	2.93	3.19	-4.49	-4.56	- .28	- .13	n.a.	n.a.
Philadelphia	-6.27	-6.07	20.49**	20.50**	11.39	11.40	1.26	1.27

Note: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice the true test statistics are usually very close to the ones reported by the regression program. Hence the significance levels given here are indicative of the actual significance levels.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

n.a. means not applicable.

IV. CONCLUSION

In this Appendix, we have investigated whether nonresponse to follow-up interviews led to biased estimates of the impact of Supported Work. Using demographic and background data obtained from a baseline interview administered to virtually all eligible applicants to the program, we estimated a model to predict the probability of response for each individual. We then used the parameters of this model to construct a new variable that, when included in the regression equation of interest, accounts for the fact that data only on the responders are available for analysis. Under reasonable assumptions, estimates of Supported Work's impact obtained from the standard regression model with this additional variable included will be free (asymptotically) of any nonresponse bias that may have been present in the unaltered regression model.

To determine Supported Work's impact on a select set of outcomes, both the standard regression equation and the augmented equation were estimated for each outcome. Comparing the alternative sets of estimates, we found little evidence of nonresponse bias. When simple models providing an overall estimate of Supported Work's impact were used, only estimates for one outcome (hours worked) showed any evidence of bias, and this evidence did not suggest any change in conclusions about the existence or general magnitude of program effects. When a more flexible model was used, which allowed estimates of program impact to vary by site, only little indication of potential bias observed. Thus, we are led to conclude that nonresponse bias is not a major problem, but the possibility of some bias should be acknowledged.

Although the conclusions above are clearly indicated from the results obtained, they are valid only if the assumptions on which the methodology is based hold. The key assumptions of this methodology, developed by Heckman (1976), are:

- That the disturbance terms in the regression and response equations are distributed as bivariate normals
- That a reliable model of the response equation is specified and estimated

A method for testing the normality assumption is not readily available, since estimates of the residuals in the response equation cannot be obtained. However, if we can do a good job of predicting response, then we have more confidence in our conclusions about whether response bias is a problem. Clearly, if we add a variable which is just random noise to the model, we would expect little change in the original coefficients, including the one on status.

Another aspect of doing a "good" job of predicting the probability of response is to identify and include variables that affect the response decision but do not affect the outcome of interest. The presence of such variables will lessen the likelihood that multicollinearity between the constructed variable ($\hat{\lambda}$) and the standard regressors (including experimental status) will confound the results.

Judging from χ^2 tests for the sets of coefficients and standard "t tests" for individual coefficients, we were able to satisfy both of these criteria. Furthermore, in every case, at least one of the variables that were included in the response equation but not in the outcome equation was found to be a statistically significant determinant of response. Thus, we have reason to believe that the model does yield reasonable predictions of the probability of response and does not introduce a high degree of collinearity into the regression model. Without actual data for the missing observations, we cannot be certain of the accuracy of our claim that nonresponse bias is minimal. However, our results do not appear to suffer from problems that we know could lead to erroneous conclusions about the presence of nonresponse bias.

APPENDIX C

THE EFFECTS OF LENGTH OF TIME SPENT IN SUPPORTED WORK ON PROGRAM IMPACTS

by Randy Brown

A key difference among Supported Work participants that could have a major impact on the effectiveness of the program is the length of time spent in Supported Work. Since individuals who dropped out of Supported Work shortly after entering might not receive the beneficial effects hypothesized to accrue to participants, analysis was undertaken of the effect of length of stay (LOS) in Supported Work on estimates of program impact.

In order to allow the estimate of experimental impact to vary with length of time spent in the program, an intuitive approach would be to regress outcomes of interest, such as, earnings, hours worked, number of arrests and drug usage, on demographic characteristics of sample members and on LOS. The experimental impact then could be measured as $\hat{\alpha}$LOS, where $\hat{\alpha}$ is the regression estimate of the coefficient on LOS.

Unfortunately, this intuitive approach to the problem may lead to erroneous conclusions. If individuals leaving the program early also tend to be the individuals with the poorest post-program performance, controlling for measured differences in personal characteristics, then the estimated coefficient on LOS will pick up not only the effects of Supported Work tenure on post-program outcomes, but also the effects of any unmeasured characteristics which affect both LOS and performance. For example, if the more motivated individuals tend to stay longer in Supported Work, and if they also tend to have higher post-program earnings, regression estimates will indicate a significant, positive impact ($\hat{\alpha}_2$) of LOS on earnings. This result will occur even if LOS, per se, has no effect whatsoever on post-program outcomes.

Statistically, the problem lies in the fact that LOS represents a behavioral decision by the participant, much like labor supply, and as such may be correlated with the regression error term, which includes the effects of all unmeasured variables (such as motivation) on the dependent variable. In this case, least squares regression produces biased coefficient estimates. This problem is referred to in the econometrics literature as "selectivity bias."

Since LOS is an endogenous regressor, an instrumental variable estimator is required to produce estimates which are asymptotically unbiased. A model was developed to explain LOS for experimentals, as a function of their personal characteristics. This model was then estimated and the results were used to obtain the predicted values for LOS. The outcome regressions included the vector of predicted values of LOS as the instrument for LOS for experimentals and zero values for controls.

The model used to predict LOS for experimentals was a Tobit model. since LOS is a bounded variable. Furthermore, since it was felt that Supported Work would have no impact on outcomes for those remaining in the program only a short time all participants remaining in the program for less than 2 months were considered to have an effective LOS of zero.

The results of the estimation of the LOS model are presented in Table C.1. They suggest that the Philadelphia site has a much lower average length of stay than the other sites, after controlling for participant characteristics. Females, youth with 10 or more years of schooling, youth raised by two parents, and those who had held some job or training prior to enrolling in Supported Work were also likely to remain in the program longer.[1/]

[1/] These results are very similar to the ordinary least squares estimates of weeks worked in Supported Work jobs, presented in Table A.6.

TABLE C.1

ESTIMATED TOBIT COEFFICIENTS AND PARTIAL IMPACTS OF VARIABLES USED TO PREDICT MONTHS EXPERIMENTALS SPENT IN SUPPORTED WORK

(Omitted variables in parentheses)

Variable	Estimated Coefficient	Estimated Effect on LOS[a]	t-ratio
Atlanta	-.92	-.71	-.74
Hartford	-1.43	-1.11	-1.57
(Jersey City)			
New York	.11	.08	.09
Philadelphia	-6.16	-4.80	-3.93
(Enrolled Before July 1976)			
Enrolled July-December 1976	-.65	-.51	-.74
Enrolled 1977	-1.13	-.88	-1.19
(Age < 17)			
Age 18	-1.16	-.90	-1.43
Age 19	-.82	-.64	-.89
Age ≥ 20	-.94	-.74	-.81
Male	-1.69	-1.31	-1.76
(Female)			
White	-.64	-.50	-.47
Hispanic	.72	.56	.81
(Black and Other)			
(< 10 Years of School)			
10 Years of School	1.65	1.28	2.28
> 10 Years of School	1.38	1.08	1.56
< 1 Year Since School	.15	.12	.19
1-2 Years Since School	.61	.48	.78
(> 2 Years Since School)			
Expelled From School or Left Because of Trouble With Law	-.99	-.77	-1.37
(Left School for Other Reasons)			
Lives with Parents	.20	.15	.28
(Does not Live with Parents)			
Raised by Two Parents	1.06	.83	1.68
(Not Raised by Two Parents)			
Married and/or Has Dependents	-1.49	-1.16	-1.45
(Not Married and No Dependents)			
Receiving Welfare or Food Stamps	.81	.63	1.04
(Not Receiving Welfare)			
(No Previous Regular Job)			
Longest Regular Job Lasted < 6 Months	1.79	1.39	2.39
Longest Regular Job Lasted ≥ 6 Months	1.31	1.02	1.42
(No Job Training in Past Year)			
Some Job Training in Past Year	1.87	1.45	2.18
(Never Used Any Drugs)			
Used Only Marijuana	.89	.69	1.24
Used Drugs Other than Marijuana	.93	.72	1.09
(Never Arrested)			
One or More Arrests	-.78	-.61	-1.04
(Not on Parole or Probation)			
On Parole or Probation	.51	.40	.60
Constant	6.71	5.22	4.08

---

[a] The effect of the ith variable on expected LOS is computed as $\hat{\beta}_i F(X\hat{\beta}/\hat{\sigma})$ where $\hat{\beta}_i$ is the estimated Tobit coefficient on the ith variable. $\hat{\beta}$ and $\hat{\sigma}$ are the estimated coefficient vector and standard error, respectively, and $F(X\hat{\beta}/\hat{\sigma})$ is the normal distribution function, evaluated at the mean value of the variables.

We then use these estimates to construct the instrument for LOS, and compute an instrumental variable estimator for the parameters on the outcome equation. Table C.2 contains the instrumental variable estimates of the effect of length of stay in Supported Work on hours worked and earnings in the 16- to 18-month period, and on hours worked, earnings, whether arrested and whether used drugs for the 19- to 27-month period. Also presented for comparison are ordinary least squares regression estimates of the effect of LOS, which as noted above, are likely to suffer from selectivity bias.

The results for the early period tend to confirm this suspicion. The least squares estimates of α (in the column headed OLS) suggest that LOS in Supported Work had a substantial and statistically significant impact on hours and earnings of participants. The coefficients imply that for each additional month spent in Supported Work, youth would work (on average) 2 additional hours per month and earn an additional 7 dollars. Thus, these estimates suggest that youth remaining in Supported Work for 6 months (roughly, the average LOS for youth) worked 12.7 more hours and earned 42 more dollars per month in the 16- to 18-month period than comparable control group members.

The instrumental variable estimates which account for selectivity bias are contained in the columns headed "IV." These estimates suggest that there is virtually no effect of LOS on outcomes in this 16- to 18-month period. Implicitly, this finding suggests that the estimated large effects effects of LOS on outcomes is due solely to differences in unobserved characteristics, such as motivation.

For the 19- to 27-month period, even the least squares estimates indicate that there is no effect of Supported Work for any length of

TABLE C.2

ESTIMATES OF THE EFFECTS OF AN ADDITIONAL MONTH IN SUPPORTED WORK
(t-statistics in parentheses)

Outcome Measure	Months 16-18		Months 19-27	
	OLS [a]	IV[b]	OLS[a]	IV[b]
Average hours worked/month	2.11*** (3.7)	-.06 (-.08)	-.11 (-.15)	-.63 (-.62)
Average earnings/ month	7.01*** (3.3)	.03 (.01)	-1.92 (-.59)	-1.97 (-.46)
Probability of arrest	n.a.	n.a.	-.0052 (-1.1)	-.0050 (-1.0)
Probability used drugs	n.a.	n.a.	-.0035 (-1.4)	-.0015 (-.35)
Number in Sample	856		413	

NOTE: These sample sizes and thus experimental-control differences differ slightly from those in the text since only observations with data on all of the dependent variables are used.

[a] OLS refers to estimates obtained by ordinary least squares regression.

[b] IV refers to estimates obtained by the instrumental variable method.

***Statistically significant at the .01 level, two tailed test.

n.a. means not applicable.

stay. The least squares and instrumental variable estimates are similar, small, and statistically insignificant.

REFERENCES

Abelson, H., Fishburn, P., and Cisin, I. National Survey on Drug Abuse: 1977, A Nationwide Study--Youth, Young Adults, and Older People, Volume 1, Rockville, Maryland: National Institute on Drug Abuse, 1977.

ABT Associates. "The Noneconomic Impacts of the Job Corps." In Assessments of the Job Corps Performance and Impacts. Washington, D.C.: U.S. Department of Labor, 1979.

Adams, A. and Mangum, G. (with W. Stevenson, S. Seninger, and S. Mangum). The Lingering Crisis of Youth Unemployment. Kalamazoo, Michigan: W.E. Upjohn Institute for Employment Research, 1978.

Anderson, B. and Young, H. "An Evaluation of the Opportunities Industrialization Center, Inc." Philadelphia: The Wharton School, University of Pennsylvania, 1968.

Ashenfelter, O. "Estimating the Effects of Training Programs on Earnings with Longitudinal Data." Presented at the Conference on Evaluating Manpower Training Programs, Princeton University, May 6-7, 1976.

Avrin, M. "The Impact of Income Maintenance on the Utilization of Subsidized Housing." Menlo Park, California: Stanford Research Institute, 1978.

Barry, F. "The Roxbury Opportunities Industrialization Center--An Economic Case Study of Self-Help Job Training in the Ghetto." Unpublished masters thesis, Cornell University, 1973.

Barton, P. and Fraser, B. (Center for Education and Work, Washington, D.C.) "Between Two Worlds: Youth Transition from School to Work. Executive Summary." Washington, D.C.: U.S. Department of Commerce, PB-286-841, 1978.

Barton, P. and Fraser, B. (Center for Education and Work, Washington, D.C.) "Between Two Worlds: Youth Transition from School to Work, Volume III, New Research and Measurement." Washington, D.C.: U.S. Department of Commerce, PB-286-844, 1978.

Becker, G. "Investment in Human Capital: A Theoretical Analysis." Journal of Political Economy, Volume 70, Number 5, Part 2, October 1962, pp 9-49.

BenPorath, Y. "The Production of Human Capital and the Life Cycle of Earnings." Journal of Political Economy, Volume 75, Number 4, August 1967, pp. 352-365.

Blew, C., McGillis, D., and Bryant, G. An Exemplary Project: Project New Pride, Denver, Colorado. Washington, D.C.: Law Enforcement Assistance Administration, July 1977.

Blinder, A. and Weiss, Y. "Human Capital and Labor Supply: A Synthesis." Journal of Political Economy, Volume 84, Number 3, June 1976, pp 449-472.

Borus, M. et al. "A Benefit-Cost Analysis of the Neighborhood Youth Corps." The Journal of Human Resources, Volume 5, Spring 1970, pp. 139-150.

Brown, R. "Assessing the Effects of Interview Non-Response on Estimates of the Impact of Supported Work." Princeton, New Jersey: Mathematica Policy Research, Inc., 1979.

Bureau of Labor Statistics, NEWS, Washington, D.C.: U.S. Department of Labor, March 9, 1978.

Cain, G. "Benefit/Cost Estimates for Job Corps." Institute for Research on Poverty, University of Wisconsin, 1968.

Chien, I., Gerard, D., and Lee, R. The Road to H: Narcotics, Delinquency and Social Policy. New York: Basic Books, 1964.

Clark, K. and Summers, L. "The Dynamics of Youth Unemployment." Cambridge, Massachusetts: The National Bureau of Economic Research, Working Paper No. 274, August 1978.

Clarke, S. "Juvenile Offender Programs and Delinquency Prevention." Crime and Delinquency Literature, September 1974, pp. 377-379.

Danzinger, S., and Wheeler. "The Economics of Crime: Punishment or Redistribution." Review of Social Economy, Volume 33, October 1975, pp. 113-131.

Dickinson, K. "Social Cost Indexes of Drug Use." Madison, Wisconsin: Institute for Research on Poverty. Discussion Paper, forthcoming 1980.

Diamond, D. and Bedrosian, H. "Industry Hiring Requirements and the Employment of Disadvantaged Groups." New York: New York University School of Commerce, 1970.

DiPrete, T. "Unemployment Over the Life Cycle: Probability Models of Turnover." Washington, D.C.: U.S. Department of Commerce, PB-287953, 1978.

Doeringer, P. and Piore, M. Internal Labor Markets and Manpower Analysis. Lexington, Massachusetts: D.C. Heath, 1971.

DuPont, R. "The Drug Abuse Scene: Past, Present, and Future." Presented at National Drug Abuse Conference, 1978, Seattle, Washington, April 5, 1978.

Erlich, I. "Participation in Illegitimate Activities." Journal of Political Economy, Volume 81, May/June 1973, pp. 521-565.

Elliot, D. and Knowles, B. "Social Development and Employment: An Evaluation of the Oakland Work Experience Program." In Conference Report on Youth Unemployment: Its Measurements and Meaning. Washington, D.C.: U.S. Department of Labor, 1978.

Engleman, R. "An Economic Analysis of the Job Corps." Unpublished Ph.D. dissertation, Berkeley, California: University of California, 1971.

Farber, D. "Highlights--Annual Follow-up: 1968, JOBS Contract and Non-Contract Programs." Report prepared for Office of Policy, Evaluation and Research, Manpower Administration, U.S. Department of Labor, 1971.

Funke, G. "Unemployment and Crime: A Socioeconomic Approach." In Crime and Employment Issues, Washington, D.C.: American University Law School, Institute for Advanced Studies in Justice, 1978.

Gay, R. and Borus, M. "Validating Performance Indicators for Employment and Training Programs." Journal of Human Resources Volume XV No. 1, Winter 1980, pp. 29-48.

Goldstein, J. "The Effectiveness of Manpower Training Programs: A Review of Research on Impacts on the Poor." In Studies in Public Welfare, No. 3, Subcommittee on Fiscal Policy, Joint Economic Committee, U.S. Congress, November 20, 1974.

Gordon, D. Theories of Poverty and Under-employment. Lexington, Massachusetts: D.C. Heath and Company, 1972.

Gramlich, E. "Impact of Minimum Wages, Employment and Family Incomes." Brooking Papers on Economic Activity, Volume 2, 1976, pp 409-461.

Greenleigh Associates, Inc. "The Job Opportunities in the Business Sector Program--An Evaluation of Impacts in Ten Standard Metropolitan Statistical Areas." Report prepared for Office of Policy, Evaluation and Research, Manpower Administration, U.S. Department of Labor, 1970.

Hammermesh, D. Economic Aspects of Manpower Training Programs. Lexington, Massachusetts: Heath Lexington Books, 1971.

Hannan, M., Tuma, N., and Groeneveld, L. "Income and Marital Events: Evidence from an Income Maintenance Experiment." American Journal of Sociology, May 1977.

Hanushek, E. and Jackson, J. Statistical Methods for Social Scientists. New York: Academic Press, 1977.

Heckman, J. "The Common Structure of Statistical Models of Truncation, Sample Selection, and Limited Dependent Variables and a Simple Estimator for Such Models." Annals of Economic and Social Measurement, Volume 5, Fall 1976, pp. 475-492.

Heckman, J. "Shadow Price, Market Wages and Labor Supply." Econometrica, Volume 42, July 1974, pp. 674-694.

Heilbrun, J. Urban Economics and Public Policy. New York: St. Martin's Press, 1973.

Hollister, R. et al. "Guidelines for Setting Wages in Supported Work." MPR Working Paper Number C-3. Princeton, New Jersey: Mathematica Policy Research, Inc., 1974.

Hollister, R., Kemper P., and Wooldridge, J. "Linking Process and Impact Analysis: The Case of Supported Work." In Qualitative and Quantitative Methods in Evaluation Research, edited by C. Reichardt. Sage Publications, Inc., forthcoming.

Jackson, R., Kueter, D., and Pannell, R. Survey Procedures and Field Results in the Evaluation of the National Supported Work Demonstration. Princeton, New Jersey: Mathematica Policy Research Inc., July 1979.

Jackson R. et al. "The Supported Work Demonstration's Research Sample: Characteristics at Enrollment." Princeton, New Jersey: Mathematica Policy Research, Inc., October 1978.

Jencks, C. Inequality. New York: Basic Books, Inc., 1972.

Jessor, R. "Predicting the Time of Onset of Marijuana Use: A Development Study of High School Youth." Journal of Consulting and Clinical Psychology, Volume 44, 1976, pp. 125-134.

Johnson, N. and Kotz, S. Distribution in Statistics: Continuous Multivariate Distributions. New York: Wiley and Sons, 1972.

Kemper, P. et al. "The Supported Work Evaluation: Final Benefit-Cost Analysis." New York: MDRC,forthcoming 1980.

Kerachsky, S. "Health and Medical Care Utilization: A Second Approach." In *The New Jersey Income Maintenance Experiment*. New York: Academic Press, 1977.

Kerachsky, S. and Mallar, C. "Design of an Evaluation of the Job Corps." Princeton, New Jersey: Mathematica Policy Research, Inc., 1977.

Kerachsky, S. et al. "The Quality of Self-Reported Data on AFDC Payments." Princeton, New Jersey: Mathematica Policy Research, Inc., 1979.

King, A. "Minimum Wages and the Secondary Labor Market." *Southern Journal of Economics*, Volume 41, October 1974, pp. 215-219.

Kirschner Associates, Inc. "Evaluation of Five Concentrated Employment Programs in Regions I and II.: New York: Kirschner Associates, Inc., 1969.

Knudsen, J., Scott, R. and Schore, A. "Household Composition." In *The New Jersey Income Maintenance Experiment*. New York: Academic Press, 1977.

Lawrence, M. "Training the Hard-Core in an Urban Labor Market--The Case of the Bedford-Stuyvesant Opportunities Industrialization Center." Philadelphia: The Wharton School, University of Pennsylvania, 1970.

Levitan, S. and Belous, R. "Reduced Worktime: An Alternative to High Unemployment." In *Job Creation: What Works?* edited by R. Taggert. Salt Lake City: Olympus Publishing Company, 1977.

Levitan, S. and Johnston, B. *The Job Corps: A Social Experiment That Works*. Baltimore: The Johns Hopkins University Press, 1975.

Louis Harris and Associates. "A Continuing Study of Job Corps Terminations: Wave II--Initial Interview with Termination from August 15, 1966 to December 15, 1966." Hearings on the Economic Opportunities Amendments of 1967. Part I. 90th Congress, 1st Session. Committee on Education and Labor, Washington, D.C., 1967.

McDonald, J. and Moffitt, R. "Uses of Tobit Analysis." *Review of Economic Statistics* (forthcoming).

Mahoney, J. "Youth Unemployment and Other Issues Affecting Violence Levels." In *Crime and Employment Issues*. Washington, D.C.: Institute for Advanced Studies in Justice, 1978.

Mallar, C. et al. "Evaluation of the Economic Impact of the Job Corps Program." In *Assessments of the Job Corps Performance and Impacts*. Washington, D.C.: U.S. Department of Labor, 1979.

Masters, S. "Using Social Security Data to Help Estimate Earnings Effects of Supported Work." Madison, Wisconsin: Institute for Research on Poverty, 1979.

MDRC. *Second Annual Report on the National Supported Work Demonstration*. New York, N.Y.: Manpower Demonstration Research Corporation, April 1978.

MDRC. *Summary and Findings of the National Supported Work Demonstration*. Cambridge, Massachusetts: Ballinger Publishing Company, 1980.

Mincer, J. *Schooling, Experiences, and Earnings*. New York: Columbia University Press, National Bureau of Economic Research, 1974.

O'Donnell, J., Voss, H., Clayton, R., Slatin, G., and Room, R. *Young Men and Drugs--A Nationwide Survey*. Rockville, Maryland: National Institute on Drug Abuse, 1976.

Ohls, James. "The Effects of the Seattle and Denver Income Maintenance Programs on the Housing Consumption of Participating Households." Draft. Princeton, New Jersey: Mathematica Policy Research, Inc., 1979.

Olympus Research Corporation. "The Total Impact of Manpower Programs: A Five-City Case Study." Volumes I and II. Report prepared for Office of Policy, Evaluation and Research, Manpower Administration, U.S. Department of Labor, 1971.

Osterman, P. "Youth, Work and Unemployment." *Challenge*, May/June 1978, pp. 65-69.

Peck, J. "The Problem of Attrition." In *The New Jersey-Pennsylvania Graduated Work Incentive Experiment Final Report*, edited by H. Watts and A. Rees. Madison, Wisconsin: Institute for Research on Poverty, University of Wisconsin, 1973.

Perry, C. et al. *The Impact of Government Manpower Programs*. Philadelphia: The Industrial Research Unit, The Wharton School, University of Pennsylvania, 1975.

Peterson, R. "An Evaluation of the Seattle Opportunities Industrialization Center." Seattle, Washington: Graduate School of Business Administration, University of Washington, 1968.

Piliavin, I. and Gartner, R. "Assumptions and Achievements of Manpower Programs for Offenders: Implications for Supported Work." Madison, Wisconsin: Institute for Research on Poverty Discussion Paper Number 541-97, June 1979.

Piliavin, I. and Gartner, R. "Supported Work: Impacts for Ex-Offenders." New York, N.Y.: Manpower Demonstration Research Corporation, forthcoming 1980.

Ragan, J. "Minimum Wages and the Youth Labor Market." *Review of Economic Statistics*, Volume LIX, May 1977, pp. 129-136.

Reynolds, M. "Crime for Profit: The Economics of Theft." Unpublished Ph.D. dissertation, University of Wisconsin, 1971.

Ribich, T. *Investing in Education to Reduce Poverty*. Washington, D.C.: The Brookings Institution, 1969.

Ruth, H. et al. "Issues of Program Design and Sampling Strategy for the National Supported Work Demonstration." Princeton, New Jersey: Mathematica Policy Research, Inc., 1980.

Schore, J., Maynard, R., and Piliavin, I. "The Accuracy of Self-Reported Arrest Data." Princeton, New Jersey: Mathematica Policy Research, Inc., April 1979.

Scott, D. "An Evaluation of the Washington Institute for Employment Training--The Opportunities Industrialization Center of Washington, D.C." Philadelphia: The Wharton School, University of Pennsylvania, 1969.

Sewell, W. and Hauser, R. *Education, Occupation and Earnings: Achievement in The Early Career*. Madison, Wisconsin: Department of Sociology, University of Wisconsin, 1974.

Silverman, C. *Criminal Violence--Criminal Justice: Criminals, Police, Courts, and Prisons in America*. New York: Random House, 1978.

Singell, L. "An Examination of the Empirical Relationship Between Unemployment and Juvenile Delinquency." *American Journal of Economics and Sociology*, Volume 26, 1967, pp. 377-386.

Sjoquist, D. "Property Crime and Economic Behavior: Some Empirical Results." *American Economic Review*, Volume 73, June 1973, pp. 439-446.

Stromsdorfer, E. "Control Group Selection." In *Conference Report in Youth Unemployment: Its Measurement and Meanings*. Washington, D.C.: U.S. Department of Labor, 1978.

Thurow, L. "Education and Economic Equality." *Public Interest*, Volume 20, Summer 1972, pp. 61-81.

Trice, H. and Roman, P. *Spirits and Demons at Work: Alcohol and Other Drugs on the Job*. Ithaca, New York: New York State School of Industrial and Labor Relations, 1972.

U.S. Department of Labor. *Assessments of the Job Corps Performance and Impacts, Volume 1*. Washington, D.C.: U.S. Department of Labor, February 1979.

U.S. Department of Labor. Conference on Youth Unemployment: Its Measurement and Meaning. Washington, D.C.: U.S. Department of Labor, October 1978.

U.S. Department of Labor. Employment and Training Report of the President. Washington, D.C.: U.S Department of Labor, 1978.

U.S. Department of Labor. Employment and Training Report of the President. Washington, D.C.: U.S. Department of Labor, 1979.

U.S. Department of Labor. A Knowledge Development Plan for Youth Initiatives. Washington, D.C.: U.S. Department of Labor, December 1978.

Voss, H. "Young Men, Drugs, and Crime." In Drug Use and Crime. Washington, D.C.: National Technical Information Services, 1976, pp. 351-368.

Westat, Inc. "Continuous Longitudinal Manpower Survey, Special Report Number 1." Rockville, Maryland: Westat, Inc., March 1979 (draft).

Winter, E. "The Businessman's Role in Closing the Gap Between Education and the Job." In The Transition from School to Work. Princeton, New Jersey: Princeton University Press, 1968.

Wooldridge, J. "Housing Consumption." In The New Jersey Income Maintenance Experiment. New York: Academic Press, 1977.

MANPOWER DEMONSTRATION RESEARCH CORPORATION PUBLICATIONS ON SUPPORTED WORK

PUBLISHED REPORTS

Ball, Joseph. IMPLEMENTING SUPPORTED WORK: JOB CREATION STRATEGIES DURING THE FIRST YEAR OF THE NATIONAL DEMONSTRATION, 1977. (Out of print)

Kolan, Nuran, AFL-CIO Appalachian Council. THE WEST VIRGINIA SUPPORTED WORK PROGRAM: A CASE STUDY, 1979.

Manpower Demonstration Research Corporation. FIRST ANNUAL REPORT ON THE NATIONAL SUPPORTED WORK DEMONSTRATION, 1976. (Out of print)

Manpower Demonstration Research Corporation. SUMMARY OF THE FIRST ANNUAL REPORT ON THE NATIONAL SUPPORTED WORK DEMONSTRATION, 1976.

Manpower Demonstration Research Corporation. SECOND ANNUAL REPORT ON THE NATIONAL SUPPORTED WORK DEMONSTRATION, 1978.

Manpower Demonstration Research Corporation. SUMMARY OF THE SECOND ANNUAL REPORT ON THE NATIONAL SUPPORTED WORK DEMONSTRATION, 1978.

Manpower Demonstration Research Corporation. SUMMARY OF THE OPERATATING EXPERIENCE AND STATISTICAL HIGHLIGHTS OF THE THIRD YEAR OF THE NATIONAL SUPPORTED WORK DEMONSTRATION, 1979.

Manpower Demonstration Research Corporation. The Final Report. SUMMARY AND FINDINGS OF THE NATIONAL SUPPORTED WORK DEMONSTRATION, 1980. (Summary version of Findings and Recommendations also available.)

Masters, Stanley H. ANALYSIS OF NINE-MONTH INTERVIEWS FOR SUPPORTED WORK: RESULTS OF AN EARLY SAMPLE, 1977.

Maynard, Rebecca A. ANALYSIS OF NINE-MONTH INTERVIEWS FOR SUPPORTED WORK: RESULTS OF AN EARLY AFDC SAMPLE, 1977.

Maynard, Rebecca A.; Brown, Randall; Schore, Jennifer. THE NATIONAL SUPPORTED WORK DEMONSTRATION: EFFECTS DURING THE FIRST 18 MONTHS AFTER ENROLLMENT, 1979.

Shapiro, Harvey D. WAIVING THE RULES: WELFARE DIVERSION IN SUPPORTED WORK, 1978.

Shapiro, Harvey D. PAYING THE BILLS: A REPORT ON THE ROLE OF LOCAL GRANTS IN FINANCING THE NATIONAL SUPPORTED WORK DEMONSTRATION, 1979.

REPORTS IN PREPARATION

Dickinson, Katherine; Maynard, Rebecca A. THE IMPACT OF SUPORTED WORK ON EX-ADDICTS.

Kemper, Peter; Long, David; Thornton, Craig. THE SUPPORTED WORK EVALUATION: FINAL BENEFIT-COST ANALYSIS.

Masters, Stanley H.; Maynard, Rebecca A. THE IMPACT OF SUPPORTED WORK ON LONG-TERM RECIPIENTS OF AFDC BENEFITS.

Piliavin, Irving; Gartner, Rosemary. THE IMPACTS OF SUPPORTED WORK ON EX-OFFENDERS.

All published reports are available from the Manpower Demonstration Research Corporation at a charge of $3.00 each to handle printing costs and mailing.

THE SUPPORTED WORK EVALUATION:
FINAL BENEFIT-COST ANALYSIS

Peter Kemper
David A. Long
Craig Thornton
Rob Hollister
Valerie Leach
Christine Whitebread
David Zimmerman

Mathematica Policy Research

CHAPTER I

THE SUPPORTED WORK DEMONSTRATION AND ITS EVALUATION

The national Supported Work demonstration, conducted in 15 sites across the country, is a special work experience program designed to help groups with well-established employment difficulties to get and keep a regular job. Other important objectives of Supported Work include reductions in welfare dependence, drug use, and criminal activity. The four target groups that provide the focus for the demonstration are (1) women who have been receiving welfare payments under the Aid to Families with Dependent Children (AFDC) program for at least three years; (2) ex-addicts who have recently been in drug treatment programs; (3) ex-offenders who have recently been released from prison or jail; and (4) young school dropouts, many of whom have records of delinquency.

In order to assess the effectiveness of Supported Work, a major evaluation component has been built into the demonstration. The effects of the Supported Work experience on the specific target groups are reported on elsewhere. This report assesses the overall effectiveness of Supported Work within the analytic framework of benefit-cost analysis.

A. THE DEMONSTRATION

Supported Work is specifically designed to be a temporary program. It provides individuals with employment for a limited time, after which they must leave, whether or not they have found jobs elsewhere. Support is provided through work assignments in crews of peers, and also through close supervision by technically qualified people who understand the work histories and personal backgrounds of their crew members. Gradually increasing standards of attendance and performance are enforced as the program proceeds, until they resemble those of regular jobs. While in the program participants earn relatively low wages, but are given opportunities to increase their earnings through bonuses and promotions for good performance and attendance.

The work done by participants, most of it relatively unskilled, is varied. It includes clerical assignments, housing rehabilitation, building maintenance, day care, and grounds maintenance, and is concentrated in the service and construction sectors. Goods and services are provided for a variety of customers, many of them in the public and private nonprofit sectors. In most of the projects participants work under the close supervision of Supported Work program staff; some, however, are outside placements in which the day-to-day supervision is provided by the host agency.

The Supported Work concept was first implemented by the Vera Institute of Justice, which started the Wildcat program in New York in 1972. The primary target groups in Wildcat were ex-addicts and ex-offenders and, by the middle of 1976, more than 4,000 had been employed in the program. The early experience with Wildcat led directly to the national demonstration of Supported Work, designed to test the concept on a larger scale and with two additional target groups--AFDC recipients and youth.

The demonstration is funded in part by grants from the Ford Foundation and a consortium of federal agencies led by the Department of Labor, and in part by locally raised funds. These latter funds come from government sources (most often CETA prime sponsors and welfare diversion payments), and from revenues generated by the sale of program output.[1]

B. RECENT EMPLOYMENT AND TRAINING POLICY AND THE DEVELOPMENT OF SUPPORTED WORK

Two major developments in employment and training policy during the last 20 years have influenced the design of both the Supported Work program and its benefit-cost evaluation: (1) the increased emphasis on targeting programs on the "disadvantaged"--persons who face difficult barriers to employment in the regular labor market--and (2) the growing reliance on work experience as an employability development mechanism. Both of these developments are reflected in Supported Work, and they provide a useful framework for understanding the program concept and the evaluation.

1. Targeting Programs for the Disadvantaged

Direct government intervention in labor markets after World War II first took the form of assistance to depressed areas of the country, largely in response to the steady decline of the dominant industries in these areas. The Area Redevelopment Act and the Public Works Acceleration Act, both passed in the early sixties, addressed the employment problems of particular areas rather than particular groups in the population. A change in policy direction came in 1962 with the passage of the Manpower Development and Training Act (MDTA). Initially, MDTA programs emphasized the training and retraining of workers whose skills were becoming obsolete because of technological and economic changes.

---

[1] See Manpower Demonstration Research Corporation (MDRC, 1978) for a detailed description of the operation of the demonstration.

In the mid-sixties the focus of the nation's training and employment policy again shifted, this time from workers with obsolete skills to individuals who had little or no job skills. With the passage of the Economic Opportunity Act in 1964, program approaches were broadened to include the use of work experience, placement assistance, and other activities in addition to skills training, in order to address the employment problems of persons with few skills and limited prospects for labor market success. During the same period, MDTA also shifted its emphasis from retraining to initial skill development and remedial education for persons with few labor market skills. By the late sixties, the nation's employment and training policy had been firmly committed to targeting programs on the disadvantaged.

Contrary to some predictions, the transformation in 1973 of employment and training programs from the existing categorical, national programs to a decentralized, decategorized set of programs under the Comprehensive Employment and Training Act (CETA) did little to change this target emphasis. Federal program regulations and local support for targeted programs helped to maintain the emphasis on serving the disadvantaged.1/ Only in a few specific cases--such as the countercyclical

---

1/While the emphasis on serving the disadvantaged is apparent in the legislation, the effectiveness of local programs in actually achieving this legislative intent has been the subject of considerable debate. Charges of "creaming"--i.e., enrolling persons with few labor market problems--have been leveled at CETA for years, and program data suggest a mixed record in providing services to those who need them most. National employment and training policy's commitment to the disadvantaged public service employment initiatives in Title VI--has this legislative and policy emphasis been suspended.

Supported Work clearly embodies this emphasis on the disadvantaged in its four target groups. Indeed, the Supported Work demonstration has served a more disadvantaged population, judging from the demographic characteristics and labor market histories of its participants, than programs funded under CETA--whose eligibility criteria are less restrictive than those of Supported Work. AFDC recipients and youth are also served by a variety of CETA programs, and AFDC recipients by the Work Incentive Training Program (WIN) as well. The two other Supported Work target groups--ex-offenders and ex-addicts--are not served in large numbers under CETA, but are generally acknowledged to face some of the most severe labor market problems of any population groups.

This emphasis on the severely disadvantaged has influenced the objectives of Supported Work and hence the design of the evaluation. In addition to its central objective of improving the employment prospects of its participants after they leave the program, Supported Work, as we have seen, seeks to reduce welfare dependence, criminal activity, and drug use, and to raise the income of participants. Given these additional objectives, the evaluation must also measure the benefits of reduced welfare dependence, criminal activity, and drug use and assess the distributional effects of Supported Work (i.e., assess the benefits and costs to the participants themselves to determine whether they are made

---

was clearly demonstrated through this history: throughout the 1970s program operators and prime sponsors have been under pressure to improve the targeting of their services.

better off). This evaluation thus differs from benefit-cost analyses of many earlier employment and training programs in which the increase in post-program earnings was typically the only benefit measured and in which distributional effects were ignored. (A review of benefit-cost analyses of other employment and training programs is contained in Chapter VIII.)

2. The Role of Work Experience

Although its importance has risen more slowly than the emphasis on the disadvantaged, the emphasis on work experience as an employability development strategy also began with the Employment Opportunity Act. Advanced as a mechanism for providing youth with initial labor market experience, work experience was initially implemented only in a few categorical programs such as Neighborhood Youth Corps, the New Career/ Public Service Careers Program, and the Concentrated Employment Program.

In the early 1970s, the policy of providing subsidized jobs received new support as a strategy for reducing countercyclical unemployment (in contrast with the earlier emphasis on structural unemployment). The Emergency Employment Act, for instance, which authorized the largest public employment program since the depression, embodied this concept. Recently, the funding for the countercyclical public service employment has been substantially reduced. The structural component of public service employment, however, has been retained in Title IID of CETA. Thus, work experience continues to be considered an important weapon in the attack on structural unemployment.

The evolution of United States policy was not the only influence on the design of Supported Work; it also has roots in Western European experience.[1] Dutch, Swedish, and British programs--such as the Social Employment Program, Industrial Workshops, and Sheltered Employment--provide government subsidized employment for persons with labor market problems. The eligibility requirements for these programs are quite broad. In addition to the mentally retarded, the eligible include "older workers, young people in trouble, former prisoners, alcoholics, and even 'querulous persons, cross grained fellows, intriguers'" (Reubens, 1970). Typical of the programs are the Swedish "sheltered workshops," which have been described as "a working environment without competition from other labour that offers a form of employment that in strain and tempo is adapted to the handicapped persons' capabilities" (OEDC, 1963, p. 21).

The demonstration, thus, exhibits both similarities with and differences from both previous United States and European experience. Its emphasis on transitional jobs makes the demonstration more similar to other U.S. work experience programs than to Western European programs, which provide not only temporary but also permanent opportunities.[2] In its strategy of carefully structuring the work experience, however, Supported Work more closely resembles the European programs. In contrast to most CETA job creation programs, in which the participant is placed with an outside organization, Supported Work takes a more intensive approach to work experience that utilizes close program supervision, graduated stress, and peer group support.[1] In implementing this approach, Supported Work often creates jobs with supervision, equipment, and materials, and all aspects of project operations under the direct control of program operators.[2] Supported Work jobs represent a much broader set of activities than CETA jobs, notably including more construction, manufacturing and revenue-producing projects. In this regard Supported Work is more similar to some of its Western European counterparts. For example, the Dutch Social Employment Program has a variety of activities, including revenue-producing industrial centers in various manufacturing activities (Haveman, 1978).

---

[1] For a comprehensive discussion of the Western European job creation programs for the hard-to-employ, see Reubens (1970), particularly Chapter IX. For a more detailed description, as well as a presentation of the results of a benefit-cost analysis, of one such program in Holland, see Haveman (1978).

[2] An exception to the general emphasis on transitional programs in the U.S. is the set of sheltered workshop programs, such as Goodwill Industries, that provide employment for disabled and handicapped workers. However, these programs are usually operated outside the formal government employment and training system.

This emphasis on carefully structured work experience, especially the use of direct supervision by Supported Work program staff, not only distinguishes it from earlier programs and many current CETA programs, but it also implies that the benefit-cost analysis must take careful account of the inputs and outputs of the jobs created to provide work experience for participants. Such direct job responsibility raises the cost of Supported Work since the projects require materials, equipment, supervision, and management for their operations. At the same time, the work experience projects produce output while participants are enrolled, and the value of this output must be measured and included as a potentially important benefit. Evaluations of earlier training programs, where the only substantial benefit (from increased employability) accrued after the program, did not face this measurement problem.

---

[1] It is interesting in this regard that Supported Work places restrictions on the use of supportive services as supplements to the work experience, limiting them to no more than 25 percent of enrollment time.

[2] In recent years more interest has been shown in the "project" concept, in which several CETA participants work together on a crew doing similar work. However, this approach is still used relatively infrequently in CETA.

C. THE BENEFIT-COST FRAMEWORK

The benefit-cost analysis reported here values the benefits attributable to the program, both during and after program participation, and compares the program's benefits with its costs. This comparison involves a large and sometimes complex set of calculations. The relative values of a wide range of outcomes--involving employment rates, welfare dependence, criminal activity, and drug use--must be compared to the cost of a wide range of resources used in operating the program. Such a comparison requires not only that the various component outcomes and costs be identified and measured, but also that they be given an appropriate dollar value so that all the diverse components can be compared.

The procedure used to compare total benefits and costs involves calculating the program's "net present value"--that is, the difference between benefits and costs where all dollar values have been adjusted for inflation and converted to present values.[1] All components in this calculation are expressed as benefits or costs per participant. The program can then be judged on the basis of its net present value per participant.

---

[1] Conversion to present values here means that all benefits and costs for all periods after the first have been discounted to the middle of the first 9-month analysis period at a 5 percent real (i.e., inflation-adjusted) annual rate. See Chapter IV for additional details.

Benefits and costs can be viewed from a variety of perspectives. Benefit-cost analyses typically take the "social" perspective, which values benefits or costs to society as a whole but ignores transfers among different groups within society. Such analyses speak to the overall economic efficiency of the program.

The social perspective is, for this reason, the most important. Other perspectives are also of interest, however, depending on the program and its objectives. Perspectives of particular regions, the budget of the funding organization, or "taxpayers" generally, are some of the other perspectives sometimes used. Because of the importance of income redistribution to programs for the disadvantaged, this report also measures benefits and costs from the perspective of participants[1/] and nonparticipants (often referred to as "taxpayers"). Net present value from the participant perspective indicates whether they were made better off on average by Supported Work--that is, whether Supported Work redistributes income to its participants.

If Supported Work redistributes income to participants, it comes from those nonparticipants who bear most of the burden of the taxes used to finance the program.[2/] Participants and nonparticipants together make

---

[1/] The term "participants" here is used to refer to those given the offer to participate in Supported Work whether or not they in fact did so. Thus, "participant perspective" is used instead of the technically correct but stylistically awkward "experimental perspective." All estimates presented below are for the experimental group as a whole, not just those who, given the opportunity, also choose to participate.

[2/] Participants also pay taxes so we have avoided the more common term "taxpayer" in referring to this group.

up society. Thus, benefits and costs from the participant and nonparticipant perspectives must sum to the corresponding benefit or cost from the perspective of society as a whole. Table I.1 presents the accounting framework for the analysis. It lists the principal components of the analysis as benefits or costs based on prior expectations concerning their impact from the social perspective (or, for those with no social impact, from the nonparticipant perspective). The second and third columns show their expected impact from the other two perspectives.[1] The participant and nonparticipant columns must, in any case, sum to the social column when the actual estimates are made.

A program can only be evaluated relative to some alternative--another program for the same target group, a set of other programs, or no program. In this evaluation, Supported Work is compared to the set of programs generally available to the target groups at the time of the demonstration--CETA, WIN, AFDC, etc. Thus, the estimated benefits and costs are not those of Supported Work compared to no program, but those of Supported Work compared to the array of conventional programs for which the target groups are also eligible. Thus, the benefits of Supported Work could fall short of costs as measured in this report because it is less effective than the array of conventional programs rather than because it is ineffective in an absolute sense.

D. ESTIMATING BENEFITS AND COSTS

Data for the analysis come from a variety of sources, which are also indicated in Table I.1. To measure Supported Work's effects, a

---

[1] For a more complete discussion of the accounting framework, see Thornton and Long (forthcoming).

TABLE I.1

EXPECTED EFFECTS OF BENEFIT-COST ANALYSIS COMPONENTS, BY ACCOUNTING PERSPECTIVE

	Accounting Perspective			
	Social	Participant	Nonparticipant	Data Source[a]
Benefits				
I. Produced by Participants				
• Value of in-program output	+	0	+	S
• Increased post-program output	+	+	0	I,P
• Preference for work over welfare	+	+	+	U
II. Increased Tax Payments	0	-	+	I
III. Reduced Dependence on Transfer Programs				
• Reduced transfer payments	0	-	+	I,P
• Reduced administrative costs	+	0	+	I,P
IV. Reduced Criminal Activity				
• Reduced property damage and personal injury	+	0	-	I,P,S
• Reduced stolen property	+	-	+	I,P,S
• Reduced justice system costs	+	0	+	I,P,S
• Reduced psychological costs	+	+	+	U
V. Reduced Drug and Alcohol Use				
• Reduced treatment costs	+	0	-	I,P
• Psychological benefits	+	+	+	U
VI. Reduced Use of Alternative Education, Training, and Employment Services				
• Reduced education and employment costs	+	0	+	I,P
• Reduced training allowances	0	-	+	I,P
VII. Other Benefits				
• Improved participant health status	+	+	+	U
• Income redistribution	+	+	+	U
Costs				
I. Program Operating Cost				
• Overhead cost	-	0	-	A,S
• Project cost	-	0	-	A,S
II. Central Administrative Cost	-	0	-	S
III. Participant Labor Cost				
• In-program earnings plus fringes	0	+	-	I,S,A
• Foregone earnings plus fringes	-	-	0	I,P
• Foregone nonmarket activities	-	-	0	U
IV. Increased Work Related Cost				
• Child care	-	-	-	I,P
• Other	-	-	0	U

NOTE: The components have been listed under "benefits" or "costs" according to whether they were expected to lead to benefits or costs from the social perspective. The contrasts between the expected effects from the social perspective and those from the participant and nonparticipant perspectives are shown by indicating, for each component, whether the net impact is to be a net benefit (+), a net cost (-), or neither (0).

[a] The codes used for data source are: S-special study, I-interview data, P-published data source, A-Supported Work accounting system data, U-item not measured.

sample of eligible applicants for Supported Work was randomly assigned either to an "experimental" group (in which case they were offered the opportunity to get a Supported Work job) or to a control group (in which case they were not). All those who went through this random assignment process were scheduled to be interviewed, initially at the time the assignment took place and subsequently at 9-month intervals for up to three years. The basic program effects (outcomes) are then measured as the differences between average experimental and control group outcomes as reflected by the data collected in the periodic interviews. That the measured program effects are based on a controlled experiment with random assignment to experimental and control groups is a very important feature of this evaluation. As discussed in Chapter VIII, most evaluations of employment and training programs have not used random assignment. This omission introduced potential biases to estimates of program effects.

The Supported Work outcomes are valued in dollars by multiplying each by an estimated dollar value per unit based on published government data, our own special studies, and the published and unpublished research of others. In estimating these dollar values, we have attempted to identify the market value of the resources used or saved, and the outputs produced by the program. We have tried to use this "resource cost" approach consistently to value benefits and costs throughout the analysis. In the case of arrests, for example, an estimate of the average criminal justice system cost per arrest (derived from the national average data and a published study of cost by type of arrest) is the value per unit, which is multiplied by the experimental-control differential in arrests to estimate one potentially important benefit--criminal justice cost savings resulting from reduced criminal activity. To value the in-program outputs produced by Supported Work projects, estimates were made of the prices that would be charged by alternative suppliers to provide the equivalent outputs.

A number of potential program benefits and costs have not been valued in the analysis, because they are either unobservable, difficult to value, or both. These include, for example, peoples' preferences for work over welfare and the potential benefit of reduced fear brought about by a reduction in crime. These unmeasured benefits and costs are indicated, however, and the effect of not measuring them is analyzed in a qualitative way.

Even for the measured benefits and costs, a great many assumptions must be made. Each assumption affects the magnitude of the estimats, and while we have attempted to make the assumptions as realistic as possible, they should not be regarded as precise accountings of social net present value. Because of this, we present both "benchmark" estimates of benefits and costs and alternative estimates that demonstrate the sensitivity of the results to key assumptions. By presenting these "sensitivity tests," we hope to reduce the likelihood of being misled by a single number. Indeed, it is not the single result of a benefit-cost analysis, but the process of establishing an accounting framework, building up the estimates of the component benefits and costs, and testing the sensitivity of the results to key assumptions that enables one to arrive at a summary judgment about the program's effectiveness.

The analysis has been limited to an evaluation of the overall effects of the program on each of the target groups. It has not addressed the differences in benefit-cost results for different program components within the demonstration--for example, whether certain types of project work are associated with higher net present value than others. As such, the analysis does not speak to questions of program replicability or improved design.

The analysis has also been limited to an evaluation of Supported Work as it was implemented in this demonstration. It has not directly addressed, for example, questions about the benefits and costs of Supported Work in a full-employment economy, or about the benefits and costs of a drastically expanded national Supported Work program institutionalized under CETA. Such extrapolation from the Supported Work evidence to unobserved environments or organizational structures would be subject to considerable uncertainty. We have attempted, instead, to base the estimates directly on evidence generated by the demonstration. Although start-up and evaluation research costs have been excluded in the benchmark estimates, benefits and costs are otherwise those of the demonstration wherever it was possible to base the estimates on the demonstration's experience. Where this was not possible, as for example in projecting benefits beyond the period when the sample was interviewed, we have attempted to make the basis for our estimates clear and to test the sensitivity of the results to alternative estimation assumptions.

The next six chapters are devoted to filling in the estimates called for in the accounting framework in Table I.1. Chapters II and III document and analyze each of the individual component estimates of costs and benefits during the first 27 months after enrollment. Chapters IV through VII present overall estimates of net present value, including projections of post-program benefits into the future, for each of the four target groups. The sensitivity of the results to various assumptions necessary for the analysis, and the implications of the unmeasured benefits and costs for the overall results, are also discussed in these chapters. Chapter VIII attempts to compare the Supported Work results with the benefit-cost results for other employment and training programs. Chapter IX is a brief summary and conclusion.

This report is one of a set of three related reports on the benefit-cost analysis. Two technical reports document the data sources and methodology used in the benefit-cost analysis. The first (Thornton and Long, forthcoming) documents the methodology and data sources used to value the effects of Supported Work, primarily the benefits discussed in Chapter III. The second (Kemper and Long, forthcoming) documents the methodology used to estimate costs and value of in-program output discussed in Chapter II. Readers interested in more detail on methodology and data sources than the relatively brief discussions here should consult these technical reports.

CHAPTER II

COSTS AND VALUE OF IN-PROGRAM OUTPUT

The costs of Supported Work have been divided, for purposes of analysis, into the following mutually exclusive categories:

Project cost is the cost of all inputs provided by Supported Work and used directly in the production of goods and services on participants' in-program jobs. These inputs include materials, supplies, transportation, equipment, and supervision.

Overhead cost is the cost of office space, salaries of staff (other than work project supervisors) and other costs incurred in raising funds, creating jobs, recruiting and later placing participants, providing supportive services, and generally managing the program and its work projects. Project and overhead cost are mutually exclusive and together make up the program operating cost (other than participant labor) at the site level.

Central administrative cost is the cost of funding Supported Work sites and monitoring the program at the national level. In public employment and training programs these functions are normally performed by the federal government; in the case of Supported Work, they are done by the Manpower Demonstration Research Corporation (MDRC), which is responsible for running the demonstration.

Participant labor cost is the in-program cost of participant labor and differs depending on the perspective taken. For nonparticipants, it is the wages plus fringe benefits paid to participants by Supported Work. For society as a whole, however, the real cost is the output participants would have produced had they not enrolled in Supported Work. Participant labor costs from both perspectives are estimated below.

Increased child care cost is incurred both during and after the program because participants work more and hence some must make alternative arrangements for the care of their children.

Unmeasured costs include increased work-related cost and foregone leisure. These are discussed but not measured in the analysis.

Value of in-program output, although not a cost, is discussed in this chapter because of its close association with costs. It is the value of the output produced on the Supported Work jobs while participants are enrolled.

As noted in Chapter I, the estimates presented here are, to the extent possible, for Supported Work as it was implemented in this demonstration. This principle ensures that the benefits and costs can appropriately be compared with each other. We depart from this principle in calculating our benchmark estimates only to the extent of excluding evaluation research and start-up costs.

Evaluation research costs have been excluded and the average employment and training program research costs substituted instead. Because Supported Work is an experiment, its evaluation has been more rigorous and expensive than the average for employment and training programs generally. To include these atypical evaluation research costs in the estimates would, in our judgment, give a misleadingly high estimate of the cost of Supported Work.[1]

Start-up costs--those additional costs incurred in starting any organization as it makes mistakes, learns, and grows to its planned size--have been excluded from the benchmark estimates on the basis of a similar, though somewhat less clear-cut, argument. How start-up costs should be treated depends on the decision being made. To the extent that such costs do not generate any additional benefits (other than getting the program underway), they are only of historical interest insofar as the present Supported Work sites are concerned. They are irrelevant to whether the benefits of continuing Supported Work are likely to exceed the costs and, hence, irrelevant to a decision to continue the demonstration sites. If higher costs during earlier periods, however, result in a more effective program--for example, if small initial scale and more intensive supervision meant greater individual attention for participants which, in turn, increased their post-program employability--then they are not entirely start-up costs and should not be excluded when deciding whether to continue Supported Work. If expanding the program by adding new sites were the issue, then start-up costs (amortized over the expected life of the sites) are relevant (although they might well be lower than the start-up costs estimated for this demonstration). Although estimated start-up costs[1] have been excluded from the benchmark estimates, estimates that include start-up costs are included as a sensitivity test.

---

[1] If the evaluation research has affected the benefits or other costs of Supported Work (for example, by improving feedback to program operators), then excluding them would be inappropriate. We do not believe, however, that the evaluation research has greatly affected the benefits and costs of Supported Work during the evaluation period.

A. PROJECT COST AND VALUE OF IN-PROGRAM OUTPUT

The projects on which participants work while enrolled in Supported Work vary widely in their supervision and nonlabor input requirements. At one extreme, the placement of a single participant in a clerical position in an outside agency under that agency's day-to-day supervision requires relatively little supervisory and nonlabor project inputs by Supported Work. At the other extreme, a gas station project run by Supported Work requires substantial nonlabor inputs--gasoline, the station itself, parts, and equipment--and detailed supervision. Although the expectation is less strong for an employment and training program than for a private firm, one would expect a project's inputs (and hence project cost) to be directly related to its output (and hence the value of that output). Indeed, one way of viewing the value of in-program output is as an offset to cost rather than as a separate benefit. To ensure consistency between the value of output and project cost estimates, the data have been obtained from the same source--a sample of case studies of work projects.

---

[1] What constitutes start-up cost is, of course, a matter of judgment. It is technically difficult to distinguish from the high cost due to small scale inevitable early in a program's development. Moreover, the length of the start-up period is not unambiguous. We have generally tried to rely on third-year data (the last year available) for the cost estimates, thus implicitly assuming a two-year "start-up" period.

1. Methodology[1]

Placing a value on the output produced by the Supported Work projects ultimately involves judgments about society's willingness to pay for the output--judgments that are inherently subjective and about which people may reasonably differ. To avoid such value judgments about society's demand for the project output, we have used as the benchmark estimate the amount an alternative supplier would charge for output equivalent to that produced by the Supported Work project. This procedure is consistent with the resource cost approach to valuing benefits and costs, which is used throughout the analysis.

The "alternative supplier's price" may not represent the amount society would, in fact, be willing to pay for the output. (The traditional example of make-work where the alternative supplier's price and society's willingness to pay differ is the work crew that digs ditches only to fill them up again. While an alternative supplier would certainly charge to perform the same work, society would not ordinarily value this activity.[2]) As a way of measuring value, however, it still has important advantages. First, the alternative supplier's price is an easily defined concept that can be measured objectively. Second, although the two are not conceptually the same, there is a theoretical justification for considering the alternative supplier's price as an upper bound estimate of the social benefit of the output produced.[1] Finally, examination of the case studies of projects in light of this theoretical argument leads us to conclude that the benefit to society from the increased output is probably not far below the alternative supplier's price.[2]

---

[1] The methodology used here is that developed by Friedman (1977); for a discussion of the theoretical framework, see also Kemper and Moss (1978); and Kemper and Long (forthcoming).

[2] Society might place a positive value on having people work, on their having increased income from wages, or other benefits of their employment. These benefits are considered separately below; "value of output" here refers to the value attached to the goods and services per se produced on Supported Work projects.

Although the alternative supplier's price is theoretically a well-dfined concept, its measurement is not always straightforward. The measurement methodology is fully documented in the technical report, and the details of specific estimates are documented in reports on the individual case studies.[1] The alternative supplier's price was measured using several different techniques:

---

[1] This economic argument, made formally and in detail in the associated technical report (Kemper and Long, forthcoming), can be summarized as follows. Two types of output are distinguished: Supported Work output that substitutes for output that would have been produced in the absence of the program and the output that represents an expansion of output beyond that which would otherwise have been produced. The mechanisms through which society benefits are quite different for the two kinds of output.

Society benefits from output that substitutes because the resources that would have been used to produce the output in the absence of Supported Work are freed to produce output elsewhere in the economy. In a smoothly functioning economy, these resources can immediately move to their most valuable alternative use, and the value of this new output will be equal to the alternative supplier's price. In an economy where there are constraints that inhibit the smooth movement of resources from one use to their most valuable alternative use, the value of the new output will be less than the alternative supplier's price.

In the case of expansion of existing output, the benefit to society is determined by society's demand for the new output. As long as the alternative supplier's price is less than the price consumers are willing to pay for the output, output should continue to expand in the absence of Supported Work. The fact that output only expands when Supported Work enters the market is thus an indication that the value of output to society must be less than the alternative supplier's price.

[2] This conclusion is based on a number of assumptions the most important of which is that resources freed by Supported Work output that substitutes for work that would have otherwise been done by regular workers will be quickly reemployed elsewhere.

Bids. When a contract was awarded to a Supported Work site through competitive bidding, the next highest bid (i.e., the lowest nonprogram bid) provided a direct estimate of the alternative supplier's price.

Revenue mark-up. When Supported Work sold its output in regular markets, the revenue received provided a basis for estimating the alternative supplier's price. The ratio of the average price charged by other firms in the market to Supported Work's prices was used to "mark-up" the Supported Work revenue to obtain an estimate of the amount an alternative supplier would charge for the same output.

Independent estimates. Where output was not actually sold, indirect estimates had to be made. One means of doing this was to have independent estimators--professional estimators in the industry in which the work was done--provide an estimate of what an alternative supplier would charge to produce the same output.

Production standards. Another indirect method relied on published estimating guides available for many industries. These guides contain estimates of the labor and materials required to perform various tasks--such as painting 1,000 square feet of wall or mowing 100 square yards of lawn. By measuring the tasks necessary for the job, determining the alternative supplier's wage rate and unit materials costs, and applying the production standards, estimates of the alternative supplier's price were constructed.

---

[1] These studies are available on request from Valerie Leach at MPR.

Relative productivity. This method relied directly on an estimate of the productivity of supported workers relative to that of regular workers. The estimated ratio was then multiplied by the hours worked by the participant to estimate the hours that regular workers would take to do the same work.[1] This estimate of hours was then multiplied by the regular workers' wage rate (marked up for fringe benefits) to estimate the amount that would have to be paid to alternative workers to produce the same amount of output.

Although conceptually simpler than estimating the alternative supplier's price, estimating project cost (the second major task of the case studies) is just as important since error in the measurement of project cost can affect the overall results as much as error in the measurement of value of output. The Supported Work fiscal and management information systems were designed so that project costs could be accounted for on an individual project basis; these accounting data then formed the basis for the cost estimates.[2]

---

[1] The relative productivity estimate is based variously on work measurements, comparison to previous labor requirements, or the assessment of supervisors.

[2] Participant work hours and wages were charged to projects on weekly time sheets, and other project costs were routinely assigned to individual projects by the program accountant. While the fiscal data formed the basis for estimating project cost, adjustments were often required for the purposes of this study to make sure that two principles were followed. (1) Project costs must correspond to the outputs valued. (For example, if a construction project took three and one-half weeks to complete, the project costs should include the cost of materials used during the same three and one-half weeks whether they were purchased specially for the project, drawn from the site's existing inventory, or contributed by the customer.) (2) Costs must be charged to projects in such a way that if all projects at a site were studied, the project cost estimated for the projects on a case-by-case basis would sum to the total

Since the projects studied varied in size and in the length of time they were studied, the value of output and project cost estimates were standardized by dividing by the number of participant hours worked on the project.[1] By estimating value of output and project cost on a per-hour basis, the results of the case studies could be averaged and ultimately related to other benefits and costs.[2]

Because value of output and project cost data are expensive to collect, detailed estimates were only made for a relatively small sample of projects. Initially, while the measurement methodology developed by Friedman (1977) was adapted to Supported Work, projects were chosen for case study judgmentally. As the methodology was extended and refined, we shifted to a random sampling of projects designed to be representative of total hours worked on projects by participants.[3]

The resulting sample consisted of 58 observations (33 judgmental and 25 random) of 44 different projects. (Ten projects were studied two or more times, accounting for a total of 14 repeated observations.)

---

project cost at the site. (This would not occur, for example, if the site failed to charge the cost of vehicles shared by several projects to the individual project accounts but instead charged them to an "indirect" cost account. In order to satisfy this "adding up" principle in the case studies, such costs were allocated to projects.)

[1] These hours include only those hours actually worked on the project; they do not include vacation, sick leave, holidays, time spent in ancillary services, or other time not spent working on the project.

[2] Measuring project hours was usually, but not always, a simple matter and it, too, is subject to measurement error. The MIS time sheets formed the basis of the estimates but they sometimes had to be adjusted for misreporting or differences in project definition.

[3] The sample selection procedures, the representativeness of the sample, and sample exclusions and reweighting are described in detail in Kemper and Long (forthcoming).

Although the sample was concentrated in the second and third years of program operations, 11 observations were studied during the first year. One would expect value of output to increase over time as sites gain experience and iron out start-up problems. Examination of results (not shown here) shows that this hypothesized increase in value of output (net of project cost) did indeed occur over time. Since start-up costs are eliminated from the benchmark estimates, projects studied during the first year of site operations[1/] were not included in the sample reported on here.[2/] In addition, one extreme outlier was excluded from the results reported here--which are, therefore, based on 46 observations.[3/]

This sample turned out not to be representative across sites. Given substantial differences among the sites with respect to the type of project undertaken, management style, and mix of target groups that might cause differences in value of output (net of project cost) across sites,

---

[1/] Some number of new projects will be initiated each year even in ongoing programs. The argument being made is that projects studied during the first year of site operations should be excluded; projects from later years studied during the first year of a specific project should not be systematically excluded from the sample.

[2/] The full sample is analyzed in Kemper and Long (forthcoming) to which interested readers can refer. In general, benchmark estimates have been based on third year costs. To do so for value of output and project costs, however, would reduce an already small sample size even further, so we have used the second and third year combined.

[3/] The excluded project, an effort by the San Francisco site to develop a solar panels installation project, was studied while the prototype was being installed, much of the work being done by a hired consultant. The project did not get beyond this initial prototype stage before it and the San Francisco site were closed. It was, thus, an unusual project which was quite small (accounting for about 0.4 percent of the hours at San Francisco during the third year). The estimate for this project lies six standard deviations below the sample mean.

we have reweighted the sample in computing averages to be representative of the actual site sizes.

2. Results

The resulting estimates are presented in Table II.1. The weighted average alternative supplier's price was $4.58 per hour and the average project cost $2.98 per hour. The difference between the two--which is what affects the overall benefit-cost results--was $1.69 per hour.[1]

The alternative supplier's price per hour varied considerably across sites. Some of this variation is due simply to differences in the project inputs across sites. Hartford, for example, had the highest alternative supplier's price per hour but it also had the highest project cost per hour; both were partly the result of the type of project undertaken at Hartford--manufacturing, repair, and retail service projects requiring extensive nonlabor inputs. Hartford's gasoline station project is an extreme example, where the cost of the gasoline itself caused both the project cost and the alternative supplier's price to be high.

Even when project cost is subtracted from the alternative supplier's price to account for these differences in inputs (see the right-hand column of Table II.1), substantial variation across sites remains apparent. The variance is, of course, even higher in the underlying data on individual projects. This high variance, in combination with the small number of observations, implies that there will be considerable uncertainty surrounding estimates of the value of output and project cost components of the benefit-cost analysis.

---

[1] This should not be interpreted to mean that the projects, on average, pay for themselves. Overhead and participant labor costs are excluded from this calculation.

TABLE II.1

ESTIMATES OF ALTERNATIVE SUPPLIER'S PRICE AND PROJECT COST PER HOUR

Site	Number of Observations	Alternative Supplier's Price per Hour	Project Cost per Hour	Alternative Supplier's Price minus Project Cost per Hour
Atlanta	4	$ 2.94	$ .39	$ 2.55
Chicago	4	2.37	.77	1.60
Hartford	13	5.85	5.08	.77
Jersey City	8	6.64	4.72	1.92
Newark	5	3.80	.77	3.03
Oakland	2	5.34	2.98	2.36
Philadelphia	7	3.98	4.55	-.57
San Francisco	3	2.82	2.82	0.00
Weighted Average[a]	46	4.58	2.90	1.69

NOTE: For definitions of terms and data source, see text.

[a] This is the average across sites weighted by the proportion of years of participant service at the sites during the third year of site operations.

The estimates thus arrived at, expressed as they are per project hour, cannot be used directly in the benefit-cost analysis or compared to overhead and central administrative cost estimates, which are expressed per year of participant service. (A year of participant service is defined as a calendar year of enrollment in Supported Work.) The per hour value of output and project cost estimates have, therefore, been converted to a per year of service basis by multiplying by a conversion ratio: the average number of project hours worked by participants per year of service.

The Supported Work Management Information System (MIS) contains data on years of participant service and hours worked on projects. Data from the third year of operation for the eight sites yielded an estimate of 1,314 project hours per year of service.[1] Using this ratio, the benchmark estimates for value of in-program output was $6,018 per year of participant service and for project cost was $3,797.

In presenting the cost estimates and the methodology used to obtain them, we proceed component by component. As stressed in Chapter I, the various components of the benefit-cost framework are viewed as benefits or costs depending on the accounting perspective used. Supported Work in-program wage payments, for example, are a cost to the nonparticipants who finance them, but a benefit to the participants who receive them. It is important to keep the different perspectives in mind while reading about specific costs.

---

[1] The estimate of total project hours has been adjusted, based on special studies, to eliminate hours worked on program-serving projects such as a janitorial project to clean the offices of the Supported Work program itself. Such projects do not directly benefit society but do so indirectly by reducing overhead costs (which reduction is accounted for in the measurement of overhead cost). Consequently, to avoid imputing a direct value of output to these projects, total project hours were adjusted downward by the proportion of total hours that were worked on program-serving projects. See the technical report for a fuller discussion of this issue and the estimation techniques.

Project cost and value of in-program output are a cost and benefit, respectively, to both nonparticipants and society as a whole. In the case of Supported Work, project cost is incurred, with minimal exceptions, by the program itself and thus is part of its budget outlays, paid largely by nonparticipants through federal taxes.

The value of output is an offset to costs from the social and nonparticipant perspectives, but it is not necessarily an offset to Supported Work budget outlays. Although customers were asked to pay for the output they received from Supported Work,[1] they often paid less than the alternative supplier's price.[2] The amount paid for the output of the projects used to make the benchmark estimate, weighted the same way, was $3,298 per year of service--a little over half the alternative supplier's price. Thus, nonparticipants received the value of output through two different routes. All taxpaying nonparticipants benefited from a reduction in the taxes required to fund Supported Work because of the revenue it received for its project output. And Supported Work's customers, also nonparticipants, benefited because they paid less than the alternative supplier's price for the project output.

Payments for project output are an alternative estimate of value of output. Customers have been shown by their payment to be willing to pay at least that amount for the project output. These payments can thus be viewed as a lower bound on society's willingness to pay for project output.[1] Supported Work revenue from the sale of project output will, therefore, be used as an alternative estimate of the value of output in the sensitivity tests.

---

[1] The Supported Work demonstration placed a great deal of emphasis on raising local funds through both grants and service project revenue so that sites were under pressure to raise as much revenue from projects as possible.

[2] A quarter of the projects in the benchmark sample earned no revenue at all.

B. OVERHEAD COST

Although the organizations set up to run Supported Work in each of the 15 sites were typically sponsored by an existing organization (for example, the Urban League in Atlanta) and sometimes continued to be closely associated with it, they were usually separate organizational structures. These organizations had to raise local matching funds, create jobs, recruit and later place participants, provide supportive services, and generally manage the program and its work projects. That all of these functions had to be performed by separate organizations serving a relatively small number of participants[2] meant that substantial costs were incurred in simply running the program. These costs of running Supported Work have been lumped together here under the single category "overhead." Specifically, overhead cost includes salaries of staff (other than work project supervisors), office space, supplies, accounting services, and supportive services.[1] Overhead here is thus more than just administrative cost (as is sometimes the case for other programs). Here overhead includes all site operating costs other than participant labor and project costs. The Supported Work fiscal system accounts for these expenditure categories separately, and data provided from these accounts by MDRC were the source of the overhead cost estimates. To enable comparison of overhead cost for programs of different sizes, they have been put on a per year of participant service basis using MIS estimates of the number and length of enrollments at each site.

---

[1] There are two reasons, discussed more fully in the technical report, why the amount paid may not be a lower bound. First, customers may make payments, ostensibly for output, that are intended partly to support the program generally. That is, they may be paying partly for other benefits (income redistribution, reduced drime, etc.). Their payment could then exceed their willingness to pay for the output per se. Second, customers' willingness to pay may not represent society's willingness to pay. The customer is often the government and society's willingness to pay must be registered through an imperfect political process, which could overstate society's demand for the output. In spite of these caveats, there are many reasons to consider it a low estimate--and we view it as such.

[2] During the third year of operations, the average number of participants enrolled ranged from 29 in San Francisco (in the process of closing) to 232 in Jersey City, with an overall average of 119.

Table II.2 shows that there is considerable variation in overhead cost across the Supported Work sites. Particularly striking is the reduction in overhead cost over time, confirming expectations of both high start-up costs and economies of scale. In line with our decision to exclude start-up costs, the average overhead costs for the eight sites

---

[1] In addition to these management expenditures, costs were also incurred for work projects that served the program itself--for example, a project providing janitorial services for the Supported Work offices. The output produced by such program-serving projects is an intermediate input consumed by the program itself. The program manager thus faces a choice between using regular staff to perform the program's functions or using participants and thus reducing management expenditures (but also reducing the total value of output produced on projects for outside customers). The project cost of these program-serving projects has been added to the management expenditures to obtain overhead cost. The project cost for program-serving projects was estimated on the basis of special studies as described in the technical report.

Rev. Shell: 10/24/79

TABLE II.2

OVERHEAD COST PER YEAR OF PARTICIPANT SERVICE, BY YEAR AND SITE

(dollars)

Site	First Year	Second Year	Third Year	All Years
Atlanta	6,801	3,186	2,946	3,734
Chicago	7,735	4,860	2,799	3,993
Hartford	7,128	4,480	3,239	4,009
Jersey City	5,984	2,839	3,363	3,539
Newark	9,022	4,091	2,257	3,778
Oakland	5,255	2,788	2,405	2,991
Philadelphia	6,956	3,501	3,587	4,241
San Francisco	7,257	6,413	6,404	6,671
All Sites	6,918	3,827	3,014	3,896

NOTE: Data are taken from the Supported Work fiscal system combined operating reports and tabulations of data from MIS status change reports. The dollar figures in this and all subsequent tables have been adjusted for inflation to the fourth quarter of 1976 using the GNP deflator.

during the third year, $3,014 per year of participant service, is used as the benchmark estimate. Since the three year average was $3,896, the implicit estimate of start-up costs (in the overhead category) is $882 per year of participant service.

C. CENTRAL ADMINISTRATIVE COST

In addition to the project and overhead costs discussed above, a program like Supported Work incurs central administrative costs--the costs of developing administrative procedures, and of funding, auditing, and monitoring the individual sites. As already mentioned, central administrative costs are usually incurred by the federal government for public employment and training programs; in the case of the Supported Work demonstration, however, most of these costs were incurred by the Manpower Demonstration Research Corporation. MDRC incurred expenses for managing the field operations, maintaining the program's fiscal system and management information system (MIS), and supporting the evaluation research.

Table II.3 presents central administrative costs for the first three years, broken down into three categories: MDRC central staff and overhead; subcontracts for the payroll, accounting, and management information systems; and the subcontracts for the evaluation research. Separating the evaluation research cost from other costs, like most functional disaggregations of costs, is not a simple matter and requires numerous assumptions. However, using data supplied by MDRC, we have made such estimates. Excluded from central administrative cost are all of the research subcontract, that part of the MDRC operations cost devoted to

TABLE II.3

MDRC CENTRAL ADMINISTRATIVE COST PER YEAR OF PARTICIPANT SERVICE

(dollars)

	First Year	Second Year	Third Year	All Years
Actual Expenditures				
Central MDRC Operations[a]	1,539	598	469	676
Fiscal/MIS Subcontracts[b]	1,521	306	140	408
Research Subcontracts[c]	3,901	1,884	1,270	1,892
Total	6,961	2,788	1,879	2,976
Expenditures Excluding Evaluation Research[d]				
Central MDRC Operations	1,297	494	379	559
Fiscal/MIS Subcontracts	868	175	69	227
Research Subcontracts	---	---	---	---
Total	2,165	669	448	786

SOURCE: Actual expenditures from MDRC (1978a), Tables VII-1 and VII-4, for the first and second years; MDRC (1978b), p. 20, and supplementary data supplied by MDRC's Fiscal and Budget Office.

[a] This category includes MDRC staff and overhead expenses incurred in connection with Supported Work.

[b] Included in this category are the cost of subcontracts for the payroll, general ledger, and management information systems.

[c] This includes the costs of the evaluation subcontracts.

[d] For details of the methods used to estimate evaluation research, see Kemper and Long (forthcoming).

evaluation research, and that part of the MIS subcontract devoted to the work-project and labor-input data required for the evaluation research. An estimate of probable evaluation research cost of a typical program, based on the average evaluation research expenditure for CETA programs--that is, 6 percent of central administrative cost--has been used instead. As can be seen in the table, the evaluation research accounted for about 75 percent of total central administrative expenditures, leaving an average over the three years of $786 per year of service.

This three-year average still includes start-up costs, which must now be excluded. Table II.3 shows that cost fell rather sharply over time largely as a result of increased scale, which supports the hypothesis that start-up cost is important during the early implementation of Supported Work. Even the third year involved some start-up due to new sites in New Jersey and Wisconsin. Based on estimates supplied by MDRC of staff salary costs by administrative function during the third year, we estimate that $83 of the $448 third year cost was devoted to start-up activities, yielding a benchmark estimate of central administrative cost of $365 per year of service. Since, if start-up cost is included, the average for all years was $786, the implicit estimate of start-up cost is $421, just over half the total.

This benchmark estimate of central administrative cost has required that assumptions be made about evaluation research, start-up, and special demonstration costs. It is useful, therefore, to compare these estimates with the central administrative costs of established programs.[1] One estimate of central administrative cost for an established program can be obtained from the Department of Labor's Employment and Training Administration's cost of managing CETA programs. An estimate of this cost for CETA public service employment was $164 per year of service in fiscal year 1977, less than half the Supported Work benchmark estimate. Differences in program purposes, operations, and scale make this estimate almost certainly on the low side, especially since many of the central administrative functions are performed or perhaps duplicated by local prime sponsors.

---

[1] See Kemper and Long (forthcoming) for the basis for these estimates.

A program more comparable to Supported Work is Job Corps. The "general administration" category of the Job Corps federal central administrative expenditures amounted to $623 per year of service in fiscal year 1977, about 70 percent above the estimate for Supported Work. Job Corps central management cost may be higher because Job Corps has substantial physical property and provides extensive residential services, which requires more central monitoring and control.

The extent of central monitoring and management is a potentially important policy choice, and the CETA and Job Corps experiences suggest the range of central administrative cost observed in established federal programs. The sensitivity of the overall Supported Work cost estimates to these alternative estimates of central administrative cost is tested later in this chapter.

D. PARTICIPANT LABOR COST

In addition to the program operating and central administrative costs discussed above, Supported Work pays participants wages and fringe benefits while they are enrolled. The outlays for wages were estimated based on interviews with participants, and marked up by 12 percent (the average Supported Work rate) to include fringe benefits. As the numbers in Table II.4 show, in-program earnings plus fringe benefits per year of service[1] were between $6,300 and $7,300. There is relatively little variation across target groups because participant wage rates varied only within a narrow range.[2]

Since participants typically stay in Supported Work for less than a year, the program wages per participant are less than the wages for a full year of enrollment. Table II.4 shows that participant wages and fringe benefits for ex-addicts, ex-offenders, and youth were about half the annual figure because they stayed in Supported Work just over half a year on average (6.8, 6.0, and 6.8 months, respectively, according to participant interviews). Program wages per participant for AFDC were higher because they stayed in Supported Work longer, 9.1 months on the average.

Unlike program operating costs, which are costs both to nonparticipants and to society, program outlays for participant wages are costs to nonparticipants but benefits to participants--and, therefore, cancel out from society's perspective. The cost to society is, rather,

---

[1] Note that participant wages per year of service cannot be compared to annual salaries because participants do not work full time due to absences, inactivations, etc. (Recall that a year of service represents enrollment time, regardless of paid time.)

[2] By design, wage rates vary somewhat across sites (depending on the entry level wage for similar workers in the city) and among participants within sites (depending on how long they were in Supported Work and whether they were crew chiefs).

TABLE II.4

PARTICIPANT LABOR COST, BY TARGET GROUP

(dollars)

	AFDC	Ex-addicts	Ex-offenders	Youth
Per Year of Service				
In-program earnings plus fringe benefits	6,473	6,770	7,274	6,304
Foregone earnings plus fringe benefits	1,175	2,188	2,232	1,734
Net increase to participants	5,298	4,582	5,042	4,570
Per Participant				
In-program earnigns plus fringe benefits	4,856	3,777	3,589	3,551
Foregone earnings plus fringe benefits	879	1,219	1,100	974
Net increase to participants	3,977	2,558	2,489	2,577

NOTE: In-program earnings plus fringe benefits per participant are the mean Supported Work earnings of the experimental group, marked up by 12 percent for fringe benefits and discounted to the mid-point of the first 9-month period at a 5 percent annual rate. Foregone earnings per participant are the mean earnings of the control group during each 3-month period multiplied by the mean proportion of the period that experimentals were enrolled in Supported Work, summed over the period and marked up by 15 percent for fringe benefits. The per year of service estimates were obtained by dividing undiscounted figures by the average length of stay (in years).

the foregone output that participants would have produced in the absence of the program. The earnings plus fringe benefits of the control group is--under customary, and we believe reasonable, assumptions[1]--an appropriate estimate of the value of this foregone output to society. These foregone earnings are estimated to correspond approximately to the period participants spent enrolled in the program.[2]

The earnings of the control group demonstrate that Supported Work has been unusually successful at targeting the program at disadvantaged workers. Foregone annual earnings plus fringe benefits ranged from about $1,000 for AFDC to $2,200 for ex-addicts and ex-offenders, with youth ($1,700) in between. These numbers show clearly that the Supported Work participants would, in the absence of the Supported Work, have had earned incomes that are low by any standard. It is also interesting to note that the earnings of the control group rose consistently over time, a trend that may be the result of improvement in the economy generally or of the gradual return to employment of the control group (who were necessarily unemployed at assignment by virtue of the program eligibility criteria), or both. Like in-program earnings, the foregone earnings per participant are lower than the annual estimates because of differences in length of stay.

---

[1] The required assumptions are: (1) Firms hire workers up to the point where the value of the additional output they produce equals their wage rate plus fringe benefits. (2) Firms and organizations accurately assess and respond to consumer demand for their output. This is especially important outside the competitive sectors of the economy and in public service employment programs. (See Chapter IV.) (3) There are no indirect labor market effects caused by Supported Work. (This issue is discussed further in Chapter IV.) (4) Illegal income does not represent a contribution of national output.

[2] Participants leave Supported Work after varying periods of enrollment so that the "during-program" period differs from participant to participant. Obtaining an estimate of what participants would have earned while enrolled in Supported Work becomes difficult because those who leave early may differ in what they would have earned from those who stay a long time. The average earnings of the control group weights the earnings of early and late leavers equally and may not be a good estimate of foregone earnings for the during-program period which should weight late leavers more heavily. Despite these complexities, we have used the average earnings of controls to estimate foregone earnings.

The specific procedure used was to estimate, for each 3-month time period, the average proportion of that three months that experimentals were enrolled in Supported Work. The proportion of the control group earnings thus arrived at was then used as the estimate for foregone earnings of participants while enrolled. (The remainder of control group earnings was assumed to be the amount that those experimentals who left the program would have earned and was subtracted from their non-Supported-Work earnings to obtain the estimate of the change in post-program earnings for that time period.) Any errors in the estimates of foregone earnings resulting from this procedure for months 1-18 will be offset by corresponding errors in the estimation of post-program earnings gains. The net present value estimates are, therefore, unaffected by this procedure.

In-program wages are a cost to the nonparticipants who are taxed to pay for them; foregone output is a cost to society; and participants gain the difference between their Supported Work earnings and what they would have earned in its absence. As Table II.4 shows, participants gain substantially in earned income as a result of being in Supported Work. The difference between the average program earnings plus fringe benefits of participants and their foregone earnings is substantial, ranging from about $4,000 per participant for AFDC to about $2,500 for the other three groups. (Participants are, of course, not made better off by the full amount of this earned income because they lose welfare payments and pay taxes.)

E. UNMEASURED COSTS AND INCREASED CHILD CARE COSTS

By limiting the estimate of the social cost of participant labor to the foregone output from paid work, the analysis ignores foregone leisure and nonmarket production, such as child care and household work. Focusing on market output is customary in benefit-cost analysis and, to the extent that the control group wants to work but cannot find jobs, the value of the leisure time may be quite low or even negative. One particularly important output that is often not paid for is child care. For the AFDC target group, the increased cost of child care is potentially large, so the increase in this work-related expense has been measured.

The differential between experimentals and controls in child care costs is estimated on the basis of interview and other data. The cost to participants is their out-of-pocket costs less any reimbursements from welfare agencies.[1] The costs of "free" day care are borne entirely by nonparticipants--as part of welfare programs.[2]

The increase in the cost of child care as a result of Supported Work was relatively small. This is not surprising since (a) one eligibility criterion for the AFDC group was that all children be at least six years old (thus presumably in school much of the time) and (b) most child care was inexpensive "informal" care--care in the homes of babysitters or relatives--rather than "formal" care--care in organized centers. This reliance on informal care reduced the cost. The difference in total day care costs between AFDC experimentals and controls during the entire 27-month period when they were interviewed was about $360, nearly half of which was borne by participants. The differential in child care costs declined over the period as a result of a decline in the difference in employment between the two groups, until it was only about $30 in the 19-to-27-month period. A rough estimate of the child care cost differential corresponding to the period when AFDC participants were enrolled in Supported Work is $200.

---

[1] Estimates were based on differentials between the experimental and control groups in the use of child care reported in participant interviews. The participant interview data, which are used to estimate nearly all benefits, are described in Chapter III.

[2] These estimates were not available directly from interview data and had to be imputed as the numbers of months children were in formal day care (from interviews) times the national average cost of formal day care (from published data), less any payments by participants for formal day care (based on interviews). See Thornton and Long (forthcoming) for details.

These costs represent only the resource cost of increased child care. They do not include possible developmental costs because the children are not cared for by their own parents. While little is known about the effects of nonparental child care on child development, this is another potential cost of putting people, especially AFDC mothers, to work. For the AFDC group served by Supported Work, these unmeasured costs may be relatively small because the age of the children ensures that child care will be limited to the period after school and vacations. If Supported Work were expanded and extended to parents with younger children, however, the cost of care of children by nonparents could become far more significant.

In addition to child care, participants face increases in other work-related expenses--such as increased transportation, clothing, and meal costs. These are costs both to participants and to society. They are probably small compared to other social costs, but they could be large enough from participants' perspective to affect the work decision of some. Whatever the case, however, no attempt has been made to measure them here.

F. SUMMARY OF ESTIMATES

Like all components of the analysis, as we have stressed, costs can be viewed from the social, participant, and nonparticipant perspectives. One additional perspective is sometimes of interest for costs--that of Supported Work's own budget. Although it is a narrower perspective than even the nonparticipant perspective, the Supported Work budget does determine the direct government expenditures required to fund Supported Work. That the same government incurs indirect costs (for example, increased child care subsidies) and gains indirect benefits (for example, reduced welfare payments) simply underscores the narrowness of this perspective and suggests caution in interpreting the estimates from the budget perspective. Nonetheless, the Supported Work budget is often viewed as the price tag for the program. If for no other reason, it is useful for comparison with the social cost estimates.

Most, but not all, of the costs to nonparticipants are part of the Supported Work budget. Project,[1] overhead, central administrative,[2] and program wage costs all appear in the Supported Work budget, but increased child care does not.[3] Much in-program output, as noted above, is sold to customers to raise revenue, which becomes part of the local matching funds and partially offsets budget outlays. The difference between the estimated value of output and the revenue received for the output is a benefit to Supported Work's customers (who are also nonparticipants).

---

[1] A small part of project cost does not appear in the Supported Work budget. Although this amount is small enough to ignore for Supported Work itself, it might not be so small for other programs, so care should be taken in making comparisons across programs.

[2] Although central administrative costs appear in the Supported Work budget in the case of this demonstration, since it is run by MDRC instead of the federal government, they might not appear in its budget under an alternative organizational structure. Again, care is required in comparing with other programs.

[3] Again under a different program design, child care might be provided as a supportive service and, hence, appear as a program budget outlay.

Table II.5 summarizes the estimates of costs and value of output per year of service, by perspective. Separate estimates of participant labor cost by target group were possible because they were based on individual interviews, data from which can be aggregated by target group. (These estimates are shown in the table.) For the other components of cost and for value of output, separate estimates by target group are much more difficult to make because the data are collected by project or site--which have varying mixtures of target groups--rather than by individual. Sample sizes, in fact, turned out to be inadequate for making reliable estimates by target group (see Kemper and Long, forthcoming). Consequently, we have used the average for all target groups.

As the table shows, total Supported Work budget outlays were about $14,000 per year of service, about a quarter of which were offset by revenue from the sale of project output. The net Supported Work budget expenditures were, therefore, estimated to be about $10,000 to $11,000 per year of service.

Net cost (that is, cost minus value of output) is substantially lower from the social perspective because the foregone output of participants was below their program wages and because the value of in-program output was estimated to be above the revenue received for it. Gross social cost averaged about $9,000 per year of service; about two-thirds

TABLE II.5

SUMMARY OF COSTS AND VALUE OF IN-PROGRAM OUTPUT PER YEAR OF SERVICE, BY ACCOUNTING PERSPECTIVE

(dollars)

	Social	Participant	Nonparticipant	
			Supported Work Budget	Outside Supported Work Budget
Project Cost	3,797	0	3,797	0
Overhead Cost	3,014	0	3,014	0
Central Administrative Cost	365	0	365	0
Participant Labor Cost				
AFDC	1,175	-5,298	6,473	0
Ex-addicts	2,188	-4,582	6,770	0
Ex-offenders	2,232	-5,042	7,274	0
Youth	1,734	-4,570	6,304	0
Increased Child Care Cost (AFDC only)	271	115	0	156
Total Cost[a/]				
AFDC	8,622	-5,183	13,649	156
Ex-addicts	9,364	-4,582	13,946	0
Ex-offenders	9,480	-5,042	14,450	0
Youth	8,910	-4,570	13,480	0
Value of In-Program Output	-6,018	0	-3,298	-2,720
Cost less Value of Output				
AFDC	2,604	-5,183	10,351	-2,564
Ex-addicts	3,346	-4,582	10,648	-2,720
Ex-offenders	3,390	-5,042	11,152	-2,720
Youth	2,892	-4,570	10,182	-2,720

[a/] Increased cost of child care is included in the total cost of the AFDC group only.

of this was offset by the estimated value of output so that net social cost per year of service was about $3,000 per year of service. As discussed above, participants enjoy a substantial benefit (which appears as a negative cost in the participant column of Table II.5) as a result of their increased earned incomes. Finally, some customers benefit directly from Supported Work to the extent that they pay less than the alternative supplier's price for project output.

The average cost per participant was again lower than the annual cost because participants average less than one year of enrollment. Table II.6 shows that the gross social cost per participant was about $6,500 for the AFDC group and around $5,000 for the other three groups. Net of the estimated value of output, social costs were thus between $1,600 and $2,000 per participant. These costs can be viewed as an investment made during the period of enrollment to earn other benefits both during and after the program.

G. SENSITIVITY OF ESTIMATES TO ASSUMPTIONS

While we believe the benchmark estimates are reasonable, a number of important assumptions had to be made in estimating them. Three areas of uncertainty in particular surrounded the estimates: (1) the exclusion of start-up costs, (2) the estimates of value of output (and the closely associated project costs), and (3) the estimates of central administrative costs. In this section, we show the effect of using some alternative estimates of these cost components on net cost.

All costs (except participant labor and child care costs) and value of output were estimated per year of service and then multiplied by the average length of stay for each target group to obtain the per

333

TABLE II.6

COST AND VALUE OF IN-PROGRAM OUTPUT PER PARTICIPANT FROM SOCIAL PERSPECTIVE, BY TARGET GROUP

(dollars)

	AFDC	Ex-addicts	Ex-offenders	Youth
Project Cost	2,845	2,117	1,872	2,136
Overhead Cost	2,260	1,681	1,487	1,697
Central Administrative Cost	270	201	178	203
Foregone Earnings Plus Fringe Benefits	879	1,219	1,100	974
Increased Child Care Cost	204	0	0	0
Total Cost	6,458	5,218	4,637	5,010
Value of In-Program Output	4,520	3,363	2,973	3,394
Cost _less_ Value of Output	1,938	1,855	1,664	1,616

NOTE: Project, overhead, and central administrative cost, and value of output were converted from year of service bases to a per participant basis by multiplying the number of months participants were enrolled in Supported Work during each 9-month period by the per month estimates (the annual estimates divided by 12) and discounting estimates to the midpoint of the first period at a 5 percent annual rate. Foregone earnings estimates were obtained from Table II.4. For AFDC the child care cost is an estimate for the period of enrollment obtained by multiplying the estimate for each 9-month period by the proportion of time spent enrolled, summing and discounting.

334

participant estimates. Since the AFDC target group had the longest length of stay, their per-participant estimates will be most sensitive to changing the estimates of cost and value of output per year of service. The other target groups had average lengths of stay roughly equal to each other, so the sensitivity of results to changes in annual cost and value of output estimates will be similar across these three groups. Although the discussion is limited to the social perspective, identical changes will apply to the nonparticipant perspective as well. (The participant perspective is unaffected by the changes considered here because participants neither pay program costs nor receive project output.)

Start-up costs. Estimated start-up costs have been excluded from the benchmark estimates on the assumption that they are incurred as part of the process of establishing an operating program and that the resources used in this process do not have a major effect on program benefits. Table II.7 presents estimates of cost and value of output that are based on averages for all three years and do, therefore, include start-up costs. The numbers in parentheses are the changes from the benchmark estimates--that is, the implicit estimate of start-up costs.

Value of in-program output is slightly lower when the start-up period is included,[1] and costs are substantially higher. If start-up costs are included in all components, the total cost net of value of output increases by about $1,700 per year of service. On a per participant basis, the AFDC net cost increases by about $1,200 and the other

---

[1] The value of output and project cost estimates in Table II.7 were based on the sample including the first year observations (but excluding the outlier).

10/26/79

TABLE II.7

SOCIAL COSTS AND VALUE OF IN-PROGRAM OUTPUT WHEN START-UP COSTS ARE INCLUDED, BY TARGET GROUP

Cost	Per Year of Service	AFDC	Ex-Addicts	Ex-Offenders	Youth
Project Cost	4,113 (+316)	3,088 (+243)	2,297 (+180)	2,032 (+160)	2,319 (+183)
Overhead Cost	3,896 (+882)	2,926 (+666)	2,177 (+496)	1,925 (+438)	2,197 (+500)
Central Administrative Cost	786 (+421)	594 (+296)	442 (+241)	391 (+213)	446 (+243)
Participant Labor	1,799 (0)[a]	879 (0)	1,219 (0)	1,100 (0)	974 (0)
Total Cost	10,594 (+1,619)	7,691 (+1,205)[b]	6,135 (+917)	5,448 (+811)	5,936 (+926)
Value of In-Program Output	5,966 (-52)	4,475 (-45)	3,329 (-34)	2,944 (-29)	3,360 (-34)
Cost Less Value of Output	4,628 (+1,567)	3,216 (+1,312)	2,806 (+951)	2,504 (+841)	2,576 (+959)

NOTE: Numbers in parentheses show the amount and direction of change from the benchmark estimates given in Tables II.5 and II.6 that results from the inclusion of start-up costs.

[a] This is an average of all four target groups.

[b] This includes $204 of increased child care cost, which was only measured for the AFDC group and is unaffected by these assumptions.

target groups by about $800. Thus, including start-up costs increases the estimates of net cost by just over a third of the benchmark.

As discussed above, the case for including all start-up costs is not strong, but an argument can be made that start-up costs should be amortized over the life of the Supported Work program and included for some purposes. If start-up costs incurred in the program's first three years are amortized over 20 years at a 5 percent discount rate, total program costs increase by less than $100 per year of service--a small relative increase.

Project cost and value of output. As discussed above, the estimates of value of in-program output and project costs were based perforce on a rather small sample of case studies. Since there is quite a lot of variance across projects in the results, the estimates of value of in-program output are subject to a good deal of uncertainty. Specifically, the estimates of value of output (net of project cost) depend on the sample exclusions and reweightings used in calculating the sample average. In addition, the benchmark assumption that the value of output to society is equal to the amount an alternative supplier would charge to produce the same output is a debatable one, although it yields a reasonable upper bound estimate. As noted earlier, the revenue actually paid to Supported Work for project output is a reasonable lower bound estimate of society's willingness to pay for the output. The sensitivity of the benchmark estimates to each of these assumptions is discussed in turn.

If the estimates had been based on the full sample without reweighting, cost net of value of output per year of service would have been $1,485 higher--an increase of about half. While we believe the exclusions and reweightings are reasonable, a different approach might have been taken to making a benchmark estimate--that is, to rely solely on the random sample (including the outlier) on the grounds that, although small, it was designed to provide unbiased estimates for the universe of project hours worked. Under this approach, net cost would have been $552 higher than the benchmark, an increase of about a fifth. Finally, if revenue had been used as the estimate of value of output instead of the alternative supplier's price, then net cost would have been $2,720 higher, nearly double the benchmark estimates of net cost. The net cost estimates are, thus, quite sensitive to the method of estimating value of output and project cost.

Central administrative costs. The estimate of central administrative costs required assumptions about the magnitude of evaluation research cost and start-up costs. If we use the estimated federal central administrative costs of the Job Corps and CETA-PSE programs, the overall estimates of social and nonparticipant cost per participant changes very little--from about $100 to $200 in either direction. Thus results are relatively insensitive to changes in the estimates of central administrative costs.

CHAPTER III

BENEFITS

In the previous chapter we have examined those elements of the accounting framework that directly concern program operation: program cost and the value of in-program output. In this chapter we consider the remaining elements of the accounting framework--the benefits that arise as participants change their behavior as a result of Supported Work. Six behavioral categories are examined: (1) the increase in output produced by participants after they leave the program; (2) the corresponding changes in tax payments; (3) corresponding changes in transfer program utilization; (4) benefits arising from reduced criminal activity among participants; (5) benefits resulting from decreases in drug and alcohol use; and (6) net resource savings from decreases in the participants' use of alternative education and employment programs during and possibly after participation in Supported Work. Each of these is discussed in turn after a brief description of the data base underlying the results.

A. DATA UNDERLYING THE BEHAVIORAL EFFECTS ESTIMATES

All estimates of the effects of Supported Work are based on data collected in periodic interviews with experimentals and controls. Everyone included in the sample analyzed in this report (and in the four target group outcome reports) was given a baseline interview at the time of enrollment.[1/] For the main sample this was followed by two follow-up

---

[1/] Not everyone in the original sample received all the interviews for which they were eligible. Those persons who missed baseline interviews were dropped from the analysis.

interviews, one administered nine months after enrollment and one 18 months after enrollment. A subsample was then administered one additional interview 27 months after enrollment. A smaller subsample was administered yet another follow-up interview 36 months after enrollment. The sample sizes of these three groups are given in Table III.1. Data from these three interview groups form the basis of the evaluation.

As can be seen from Table III.1, the sample sizes of those who were followed for the full 36 months (the 36-month cohort) are small--and for the AFDC group zero. For this reason, and also because the 36-month cohort appears to have behaved somewhat differently from the other cohorts throughout the analysis period, the estimates derived for the period after month 27 must be interpreted with care. The basic benefit results, therefore, are presented for months 1-27. The results for the later period are discussed in the target group chapters when the issue of extrapolation to future periods is addressed.

Two other basic features of the methodology used in the benefit-cost analysis should be mentioned before turning to the results. First, effects are estimated as differences in mean values between controls and experimentals (or vice versa) for some benefits. The second common feature is that in order to adjust for inflation over the period of the demonstration, 1974 to 1978, all values (as was true for the costs and value of output) are expressed in terms of fourth quarter 1976 dollars--the approximate midpoint of the evaluation.

B. INCREASED POST-PROGRAM OUTPUT

A major objective of Supported Work is to increase the output produced by participants after they leave the program. Such increases

TABLE III.1

NUMBER IN SAMPLE, BY TARGET GROUP AND TIME PERIOD

Months since enrollment	AFDC	Ex-addicts	Ex-offenders	Youth	Total
Months 1-18[a]	1,351	974	1,479	861	4,665
Months 19-27[b]	616	885	1,011	513	3,025
Months 28-36[c]	0	311	292	153	756

NOTE: For some variables, missing data may cause the sample sizes to be slightly smaller than shown.

[a] The 18-month sample includes all who completed a baseline, a 9-, and an 18-month interview.

[b] The 27-month sample includes all who completed a baseline and a 27-month interview, whether or not they completed 9- and 18-month interviews.

[c] The 36-month sample includes all who completed a baseline and a 36-month interview, whether or not they completed the intervening interviews.

in the production of goods and services for the economy as a whole (i.e., increases in gross national product or GNP) are important potential benefits to society. They will also be seen as benefits by participants who receive the increased wages corresponding to the increased production. In addition, increased employment is presumed to affect welfare dependence, drug abuse, criminal activity, and the use of alternative education and training programs (effects that will be valued separately in the following sections).

The increase in post-program output due to Supported Work is the difference between what experimentals actually produce after leaving the program and what they would have produced had they not enrolled in Supported Work. This difference is estimated as the difference between the average gross compensation of experimentals (i.e., their pretax wages plus fringe benefits) and that of controls. This requires, first, that the value of output can be estimated from earnings. This assumption is standard in microeconomic analyses of this kind, but will be examined more closely in the chapters that report the target group results (Chapters IV through VII). It requires, second, that the gross compensation of controls be a good estimate of what experimentals would have earned in the absence of Supported Work. The design emphasis on random assignment to the experimental and control groups allows us to make this second assumption.

Table III.2 presents the estimates of the net increase in post-program earnings plus fringe benefits during the first 27 months following enrollment for the four Supported Work target groups. During months 19-27, when nearly all participants had left Supported Work, the earnings increase

TABLE III.2

INCREASED POST-PROGRAM EARNINGS PLUS FRINGE BENEFITS

(dollars)

Time Period	AFDC	Ex-addicts	Ex-offenders	Youth
Months 1-9	59	-132	129	58
Months 10-18	401	-111	80	-126
Months 19-27	627	92	106	65
Total (months 1-27)	1,087	-151	315	-3

NOTE: The increase in post-program earnings is estimated by the mean non-Supported-Work earnings of experimentals minus the mean earnings of controls, all marked up by 15 percent for fringe benefits. For months when some experimentals were still enrolled in Supported Work, mean earnings of controls were multiplied by the mean proportion of the period that participant were not enrolled in Supported Work.

due to Supported Work for the AFDC target group was substantial--over $600. For ex-addicts, ex-offenders, and youth, however, earnings increases were quite small, indicating that Supported Work had little effect on their post-program employability as measured by post-program earnings.[1]

C. INCREASED TAXES PAID BY PARTICIPANTS

As earnings of experimentals rise, so do the taxes they pay. Participants see this as a cost; but nonparticipants, who are the major beneficiaries of the increased taxes, see them as a benefit. From the perspective of society as a whole the benefits and costs to these groups will cancel out, so tax payments will not enter the social benefit-cost calculations.

Table III.3 presents estimates of the increased tax payments for each of the four target groups. They include tax payments for federal

---

[1] Problems arise in attempting to estimate changes in post-program earnings for the time periods when many participants are still in Supported Work, stemming from the difficulties of making valid comparisons between a subgroup of experimentals (those who have left the program) and the entire control group. This means that the estimates for months 1-18 are not as reliable as those for the later period. The mirror image of this problem was confronted in estimating foregone earnings for the "in-program" period. As discussed in Chapter II, the specific procedure used was to estimate, for each 3-month period, the average proportion of the three months that experimentals were enrolled in Supported Work. That proportion of the control group earnings was used as the estimate for foregone earnings of participants while enrolled. The remainder of control group earnings was assumed to be the amount that those experimentals who left the program would have earned; it was subtracted from their non-Supported-Work earnings to obtain the estimate of the change in post-program earnings for that time period. Any errors in the estimates of post-program earnings gains resulting from this procedure for months 1-18 will be offset by corresponding errors in the estimates of foregone earnings. The net present value estimates are therefore unaffected by this procedure.

TABLE III.3

INCREASED TAX PAYMENTS PER PARTICIPANT

(dollars)

Time Period	AFDC	Ex-addicts	Ex-offenders	Youth
Months 1-9	343	233	408	265
Months 10-18	139	27	95	28
Months 19-27	83	33	50	8
Total (Months 1-27)	565	293	553	301

NOTE: These figures have been discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

and state income taxes, social security payroll taxes, and sales and excise taxes. Taxes have been imputed on the basis of statutory tax incidence (by state of residence where appropriate) and average consumption patterns. They take into account not only total income but also household size, relevant tax rates and regulations, and national data on household expenditures by income class.

The total increase in taxes paid by participants was substantially larger for months 1-9 than for the later periods. This follows the pattern of participant incomes, which were highest during the first period when most participants in the demonstration were earning program wages. In later periods income fell as participants left the demonstration. Taxes paid by AFDC participants increased the most; we estimate that they paid a total of $565 per participant more than AFDC controls over the whole 27-month period. Ex-offenders had the second largest increase, $553 per participant, while ex-addicts and youth had similar increases (around $300). Most of these tax increases were due to increased payments for the federal income tax and the social security payroll tax.

D. REDUCED USE OF TRANSFER PROGRAMS

The benefit-cost analysis divided the benefits from reduced use of transfer programs by participants into two components: the reductions in the dollar value of transfer payments to participants and the reduction in the administrative costs of transfer programs. Nine types of transfer programs were assessed. These included five programs providing direct cash transfers--Aid to Families with Dependent Children (AFDC), general assistance (GA), unemployment compensation, Social Security, and

Supplemental Security Income (SSI); three programs providing in-kind transfers--food stamps, public housing, and Medicaid; and other welfare, a category which includes cash transfers from miscellaneous small programs plus those cases where individuals reported receiving cash transfers (probably from AFDC or general assistance) but were unable to say from which specific program the payments came.

1. Reduced Transfer Payments

For participants, the net reduction in average transfer payments represented one of the major costs of participating in Supported Work. In fact, for the AFDC target group it was (on average) the largest cost, even larger than the foregone earnings. For the other target groups, the reductions in transfers were smaller and represented less of a cost to them. For nonparticipants, the reductions represented a benefit in the form of a lower tax bill. From the social perspective, the gains and losses brought about by the change in transfers again cancel out. Thus, as with taxes paid, the changes in transfers are relevant for evaluating the distributional consequences of Supported Work but not its social efficiency.

Self-reports of transfer payments received by respondents (obtained from the interviews) were used to estimate the changes in payments to participants from the cash transfer programs. To estimate changes in in-kind transfer programs, and changes in other household members' use of cash welfare programs, however, it was necessary to use imputation procedures. These imputation procedures involved estimating changes in program utilization obtained from the interviews and valuing these changes using published data regarding average benefit amounts.[1] Where possible, account was taken of major household characteristics affecting program eligibility and payments levels.

The estimated reductions in the average transfer payments received by participants are shown in Table III.4. As mentioned, they indicate that all target groups had an overall reduction in transfer payments. The differences in the magnitude of these reductions reflect the differences in the extent to which the various target groups were eligible for and would have participated in the transfer programs. The AFDC group had the highest initial level of transfer use and, correspondingly, showed the largest reduction (over $2,600 per participant). The other groups had lower initial levels of use and, therefore, the reductions were smaller. The estimated reductions in total transfer payments per participant to the three non-AFDC groups were $530 for ex-addicts, $219 for ex-offenders, and $474 for youth.

The largest payment reductions over the period involved the cash transfer programs--AFDC, general assistance, and other welfare--although

---

[1] In the case of food stamps, an estimate of the total bonus value (the difference between what the stamps were worth and what the household paid for them) was made on the basis of self-reports of the amount received. For those respondents who reported receiving food stamps but could not specify the amount, an estimate was made based on data on months receiving food stamps, household size, and income. For public housing the imputation was based on the difference between published estimates of average market values of the housing units and the actual rent paid by the household. The value of the Medicaid transfer was computed using reported doctor visits and hospitalizations that were covered by Medicaid and estimates of the national average costs of doctor and hospital care. To estimate other household members' receipt of cash transfers we used estimates (based on interview data) of the number of months other household members received payments from each program. These estimates were then multiplied by the average benefit levels of the various programs in order to obtain estimates of the value of the change in transfers.

TABLE III.4

CONTROL-EXPERIMENTAL DIFFERENTIALS IN TRANSFER PAYMENTS PER PARTICIPANT FOR MONTHS 1-27, BY TARGET GROUP

(dollars)

	AFDC	Ex-addicts	Ex-offenders	Youth
AFDC	2,072	274	102	129
General Assistance (GA)	10	218	120	130
SSI	-30	23	-8	1
Other Welfare	15	1	25	29
Unemployment Compensation	-280	-188	-47	-146
Social Security	56	41	-3	4
Medicaid	301	127	-57	34
Food Stamps	406	72	51	31
Public Housing Subsidy	24	-1	6	20
AFDC to Other Household Members	37	-69	22	213
GA to Other Household Members	3	28	19	-15
Other Welfare to Other Household Members	1	3	-11	45
Total (Months 1-27)	2,615	530	219	474

NOTE: These figures are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

the AFDC group also lost a substantial amount of Medicaid and food stamps. Again, the reductions were greatest for the AFDC target group and smaller for the other groups. It is interesting to note that the reductions in AFDC are very similar to reductions in general assistance payments for all groups except AFDC, for whom effects on general assistance are minimal. The effect of Supported Work on the receipt of AFDC and general assistance by other household members was mixed, with the largest effects being for AFDC payments to other household members for the youth group.

The remaining reductions were concentrated in the in-kind transfer programs--food stamps, Medicaid, and public housing. Changes in receipt of Social Security and SSI were small and varied in magnitude and sign.

The only major exception to the overall pattern of reduction in transfers received was unemployment compensation, which increased for all target groups. In months 1-9, experimentals had lower average unemployment compensation payments. However, their employment in Supported Work during that period made some participants eligible for such compensation in later periods (regular Unemployment Insurance in New York and Special Unemployment Assistance in the other sites) and caused average unemployment compensation to rise in the later periods for all target groups.

2. Transfer Program Administrative Costs

While reductions in transfer payments do not enter the social benefit-cost calculation, the associated changes in transfer program administrative costs do. Any effect of Supported Work on resource use must be included in the accounting from the social perspective, and any change in the amount of resources needed to run the transfer programs is included in this category.

For each of the cash transfer programs and also for food stamps, the average change in administrative cost per participant was computed by multiplying the average change in the number of months participants received transfers by the average administrative cost per case month. The data on transfer program use came from self-reports gathered in the interviews, while the average cost data were obtained from federal government budget data.

In the case of public housing, the same procedure was followed with one exception. Estimates of average administrative costs were obtained from a study of the housing assistance supply experiment (Lowry, 1978). This procedure was followed because it was necessary to use an estimate of average administrative costs that included only the costs of providing the transfer, not the costs of managing the public housing units (which are included in the value of the public housing transfers presented in Table III.4).

The administrative costs of Medicaid were measured using Medicaid budget data to estimate the ratio of total administrative costs to total benefit payments, which was then multiplied by the estimated Medicaid transfers received by sample members.

The pattern of the estimated administrative cost savings due to the reductions in transfer program participation among supported workers is shown in Table III.5. The AFDC target group generated the largest reduction in transfer program administrative cost ($137), followed by youth ($78), ex-addicts ($47), and ex-offenders ($41).

TABLE III.5

CONTROL-EXPERIMENTAL DIFFERENTIALS IN ADMINSTRATIVE COSTS OF TRANSFER PROGRAMS PER PARTICIPANT FOR MONTHS 1-27, BY TARGET GROUP

(dollars)

	AFDC	Ex-addicts	Ex-offenders	Youth
AFDC	102	23	11	12
General Assistance (GA)	0	29	16	16
SSI	0	2	-2	2
Other Welfare	1	-1	5	5
Unemployment Compensation	-35	-17	-4	-14
Social Security	1	1	0	0
Medicaid	14	6	-3	1
Food Stamps	42	7	5	8
Public Housing Subsidy	7	1	10	20
AFDC to Other Household Members	4	-8	3	25
GA to Other Household Members	0	3	2	-2
Other Welfare to Other Household Members	0	0	-1	5
Total (Months 1-27)	137	47	41	78

NOTE: These figures are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

E. REDUCED CRIMINAL ACTIVITY

The methodology used to value the effect of Supported Work on participant criminal activity takes the estimated reductions in crime and multiplies them by the estimated social value of such reductions.[1] This procedure is done for several different types of crime so that changes in both the overall level and the mix of criminal activity can be assessed. The estimates of crime reductions are based on estimates of the control-experimental differences in average number of arrests. The estimates of the social value of crime reductions are based on the value of the resources society saves when crimes are not committed--specifically, the resources saved from reduced costs of the criminal justice system (police, prosecution, courts, and corrections), reduced personal injury and property damage, and reduced stolen property. Society's demand for crime reduction (i.e., its willingness to pay) is not directly measured by this procedure, an issue discussed later in this section.

The resource savings from reduced personal injury, property damage, and criminal justice system costs will be benefits to society and to nonparticipants, but will not affect participants.[2] The reduced value of stolen property will also be a benefit to nonparticipants and society, but less so to society to the extent that gains to nonparticipants are offset by losses to the participants who no longer receive the stolen property. Thus, the social benefit will consist of the difference between the nonparticipant benefit and the cost to participants. This difference is due primarily to fencing costs, damage to the stolen property, and the loss in value because stolen property does not carry a legal title.

---

[1] This procedure assumes that other criminals do not change their behavior in response to the reduction of crime among program participants (they do not "replace" participants in the criminal activities that participants forego). That is, the reduction in criminal activity among participants is assumed not to make it significantly more profitable for other persons to enter into illegal activities and replace the participants.

[2] Because some participants would be expected to be victims of crime, some of the reduction in victims' losses should be counted as a benefit to participants. This gain is likely to be very small, however, because relative to total population the number of participants is small. The entire savings is therefore treated, for analytical purposes, as a gain to nonparticipants.

1. Measuring Criminal Activity

The first step in valuing the reduction in criminal activity is to measure that reduction.[1/] However, it is difficult to obtain accurate measures of an individual's criminal activity because people engaged in such activity have an incentive to hide their actions. To get around this problem we used self-reports of arrests as the basis of our measure of criminal activity.[2/] Arrests are typically well-defined events, so the interview respondents should have a clear understanding of what they were being asked to report. Also, official records are kept of arrests so that

---

[1/] Criminal activity interview questions were not asked of members of the AFDC target group because of a prior assumption that there would be negligible crime-related effects associated with the AFDC group. As a result, only the ex-addicts, ex-offenders, and youth target groups are included in the benefit-cost crime analysis.

[2/] One alternative proxy measure that was not used was convictions for crimes. This measure would reduce the problems associated with arrests of individuals for crimes they did not commit. However, it has other serious shortcomings. Because of plea bargaining and problems with evidence, the charge on which a person is convicted may not reflect the seriousness of the crime actually committed. More importantly, because the evaluation's observation period is finite the use of judicial outcomes (such as conviction) to measure short-run changes in criminal activity may fail to capture a program's effect on the more serious crimes because arrests for these crimes often take a long time to adjudicate fully. Moreover, so long as the ratio of convictions to arrests is not changed by participation in Supported Work, both measures will give equally good measures of crime benefits.

verification studies can be made, and their results used to adjust for underreporting. Such a verification study was conducted for a subsample of experimentals and controls at the Hartford, Oakland, and San Francisco sites (Schore, Maynard, Piliavin, 1979). It found evidence of substantial underreporting by experimentals and controls, but essentially no significant difference in the extent of underreporting between the two groups. The estimate of the ratio of officially recorded arrests to self-reported arrests derived from this study is approximately 1.7. The estimate of control-experimental arrest differentials is, therefore, multiplied by this ratio so that the estimated benefit is based on corrected estimates of the arrest differentials.[1]

We also corrected for the fact that many crimes do not result in arrests. This was done by using data from victimization studies, where appropriate, to adjust the estimates of social cost per criminal incident to a per arrest basis. The adjustment involves multiplying the per-incident estimates by the ratio of criminal incidents to arrests for each of the eight arrest categories. The result will be an accurate estimate of the effects of Supported Work on criminal activity as long as the true ratio of incidents to arrests is relatively constant and independent of participation in Supported Work.

The interview data were aggregated into eight crime categories--murder and felonious assault, robbery, burglary, larceny and motor vehicle theft, drug law violations, other personal crimes, other miscellaneous crimes, and an unspecified category for those cases where specific charges were not obtained. This aggregation was done on the basis of the most serious charge reported for each arrest, estimated on the basis of resource costs and measures of social concern regarding the charge (e.g., the Sellin-Wolfgang [1974] index).

---

[1] The effect of dropping the underreporting correction is examined later in this section.

The estimated arrest differentials (adjusted for underreporting) are reported in Tables III.6 through III.8. The largest effects were found for the ex-addict target group. Not only did this group appear to reduce its overall level of criminal activity (compared to what it would have been in the absence of Supported Work), but it also shifted away from the more serious crimes, particularly robbery, burglary and drug law violations. The changes in total arrests and in the mix of crimes were much smaller for the ex-offender and youth target groups, and not statistically significant.1/

As we will see, differences in these crime results explain much of the difference in the final benefit-cost estimates of these three target groups. The large reductions in arrests for ex-addicts generated substantial benefits and lead us to conclude that the demonstration was a success for this group. For youth the absence of any real crime benefits (in addition to the lack of post-program earnings effects) results in a negative assessment of the program for them. In the case of ex-offenders, the absence of any clear-cut crime effects lies at the heart of the uncertainty regarding Supported Work's effectiveness for that group.

1/ See Piliavin and Gartner (1979) and Maynard (1979).

TABLE III.6

CONTROL-EXPERIMENTAL DIFFERENTIALS IN ARRESTS PER PARTICIPANT, BY MOST SERIOUS CHARGE AND TIME PERIOD

EX-ADDICT SAMPLE

	Months 1-9	Months 10-18	Months 19-27	Months 1-27
Murder and Felonious Assault	-0.012	0.022	-0.009	0.001
Robbery	0.066	0.022	-0.008	0.080
Burglary	0.021	0.031	0.027	0.079
Larceny and Motor Vehicle Theft	-0.014	-0.007	-0.020	-0.041
Drug Law Violations	0.000	0.037	0.043	0.080
Other Personal Crimes	-0.002	-0.002	0.000	-0.004
Other Miscellaneous Crimes	-0.047	0.016	0.024	-0.007
Unspecified Crimes[a/]	-0.009	-0.016	0.001	-0.024
Total Arrests	0.003	0.103	0.058	0.164

[a/] Arrests for which charges were not reported are classified as unspecified.

TABLE III.7

CONTROL-EXPERIMENTAL DIFFERENTIALS IN ARRESTS PER PARTICIPANT, BY MOST SERIOUS CHARGE AND TIME PERIOD

EX-OFFENDER SAMPLE

	Months 1-9	Months 10-18	Months 19-27	Months 1-27
Murder and Felonious Assault	-0.036	0.008	0.017	-0.011
Robbery	0.001	0.013	-0.019	-0.005
Burglary	0.014	-0.021	-0.026	-0.033
Larceny and Motor Vehicle Theft	-0.010	-0.058	-0.034	-0.102
Drug Law Violations	-0.022	0.003	-0.008	-0.027
Other Personal Crimes	0.027	-0.012	0.020	0.035
Other Miscellaneous Crimes	0.054	-0.020	-0.002	0.032
Unspecified Crimes[a]	0.024	0.025	-0.114	-0.065
Total Arrests	0.052	-0.062	-0.166	-0.176

[a] Arrests for which charges were not reported are classified as unspecified.

TABLE III.8

CONTROL-EXPERIMENTAL DIFFERENTIALS IN ARRESTS PER PARTICIPANT, BY MOST SERIOUS CHARGE AND TIME PERIOD

YOUTH SAMPLE

	Months 1-9	Months 10-18	Months 19-27	Months 1-27
Murder and Felonious Assault	-0.001	0.001	-0.024	-0.024
Robbery	0.018	0.004	0.014	0.036
Burglary	0.015	-0.003	0.002	0.014
Larceny and Motor Vehicle Theft	-0.026	-0.043	0.044	-0.025
Drug Law Violations	-0.021	0.005	-0.015	-0.031
Other Personal Crimes	-0.012	0.013	0.018	0.019
Other Miscellaneous	0.023	-0.015	0.027	0.035
Unspecified Crimes[a]	-0.055	0.009	0.025	-0.021
Total Arrests	-0.059	-0.029	0.091	0.003

[a] Arrests for which charges were not reported are classified as unspecified.

2. Valuing Changes in Criminal Activity

To estimate the value of changes in criminal justice system costs, personal injury and property damage, and stolen property, these differences in mean number of arrests per participant were multiplied by estimates of the cost per arrest for the different types of crime. The estimates of cost per arrest are described in this section and the estimated values of the Supported Work crime benefits are discussed in the following section.

Reduced criminal justice system costs. The largest measured component of the resource savings from reduced crime is the savings to the criminal justice system. The costs of apprehending, adjudicating, and incarcerating criminals are substantial, as is shown by the estimated average costs per arrest shown in the first column of Table III.9.[1] Therefore, even relatively small reductions in arrests can yield substantial social benefits, especially for murder and felonious assault, robbery, and burglary. Thus, reductions in arrests for serious crimes, as was observed for ex-addicts, generate large benefits.

---

[1] These costs were derived from a study of justice system costs in Baltimore, Maryland (Lettre and Syntax, 1976). This study broke down total system costs by major subsystem--police, detention, district court (for preliminary hearings and misdemeanors), Supreme Court bench (appeals and felonies), and corrections--as well as by crime type. Because the study also included data on the number of people arrested for each crime type, it was possible to estimate average costs per arrest for the different crime categories. The average cost of an unspecified arrest was taken to be the weighted average cost for all arrests. While the Baltimore data probably reflect the relative costs of the different arrest charges, they may be inaccurate for nationwide studies because they were obtained from a single jurisdiction. Therefore, national cost estimates regarding the average cost of an arrest were used to adjust all the Baltimore figures. This adjustment also incorporated a factor to correct for inflation between 1974 (the year of the Lettre and Syntax data) and the fourth quarter of 1976.

TABLE III.9

AVERAGE SOCIAL COST OF CRIME PER ARREST

(dollars)

	Criminal Justice System Costs	Personal Injury and Property Damage Costs[a]	Stolen Property Resource Costs[b]	Total Cost Per Arrest
Murder and Felonious Assault	4,338	7,782	0	12,120
Robbery	12,087	569	479	13,135
Burglary	5,895	537	2,317	8,479
Larceny and Motor Vehicle Theft	2,618	408	1,268	4,294
Drug Law Violations	2,590	0	0	2,590
Other Personal Crimes	756	94	0	850
Other Miscellaneous Crimes	919	0	0	919
Unspecified Crimes[c]	2,048	171	348	2.567

[a] The drug law violations and other miscellaneous crimes categories contain primarily "victimless" crimes. This implies direct losses to victims are small; hence a value of zero is assumed.

[b] Stolen property social costs, estimated only for property crimes, are estimated as a fraction (65 percent) of the average value of property stolen per arrest.

[c] The unspecified crimes category contains arrests for which no charge was recorded. Costs for this category are estimated as the weighted average of the costs of the other crime categories.

Reduced personal injury and property damage. The second measured component of the benefits from reduction in crime is the resource savings from reductions in the amount of crime-related personal injury and property damage. Using data from the National Crime Panel Survey program and other sources, we were able to obtain estimates of (1) the average value of property damage from criminal acts, (2) the average value of the medical care needed by victims of personal crimes, (3) the average output lost when victims lose time from work while they are recovering from personal crimes, and (4) the average costs of the administration of insurance needed to compensate victims.[1]

Cost estimates were again made for each crime type so that changes in both the level and composition of criminal activity could be valued. In addition, the cost estimates were adjusted to reflect the number of incidents per arrest for each type of crime. Thus, the value placed on the different arrest types reflects the expected resource savings generated by a reduction in one arrest of that type. These values are shown in the second column of Table III.9.

Reduced stolen property. Estimates of the average value of property stolen per incident were made using a methodology similar to that used to obtain the personal injury and property damage estimates. They reflect victims' self-reports of the market value of goods stolen and not recovered (as with the other crime victimization estimates, these were taken from the National Crime Survey data base). The per-incident values were adjusted to a per arrest basis by multiplying the ratio of incidents to arrests for each crime type. Thus, we were able to estimate the value of the expected property loss per arrest for the property crimes--robbery, burglary, larceny and motor vehicle theft (with some allowance for the presence of these types of crimes in the unspecified arrest category). The expected loss estimates are shown in the first column of Table III.10.

---

[1] We would like to thank Wesley G. Skogan for his help in obtaining and interpreting the necessary estimates from the victimization-incident data gathered as part of the National Crime Panel program.

The distributional consequences generated by changes in the amount of property stolen by participants are more complicated than those of other crime-related effects. Nonparticipants view a reduction in stolen property as a benefit (because the goods are no longer stolen from them), while participants view it as a cost (because they no longer receive the stolen goods). However, the benefits to nonparticipants are unlikely to equal the cost to participants. For example, if thieves try to convert stolen goods into cash, they will be able to realize, on average, only about 35 percent of the goods' value to the victims of theft (see Drug Enforcement Administration, 1977). Furthermore, as we have already mentioned, there may be a decline in the social value of the goods because (1) the goods may be damaged; (2) the thief (and whoever else ultimately receives the stolen property) does not have legal title to it; and (3) resources (labor and materials) are used up in fencing and related activities associated with selling stolen property. These factors imply that the social cost of stolen property can be approximated by the differences in the value of the goods before and after they have been stolen (i.e., the difference between their value to the nonparticipants and to the participants). The distributional implications of the changes in stolen property are shown in Table III.10. The final column presents

TABLE III.10

VALUE OF STOLEN PROPERTY PER ARREST

(dollars)

	Expected Value of Property Loss to Victim	Expected Value of Property to Criminal	Net Social Loss
Robbery	738	259	479
Burglary	3,564	1,247	2,317
Larceny and Motor Vehicle Theft	1,951	683	1,268
Unspecified Crime[a]	536	188	348

---

[a] The unspecified crime category contains arrests for which no charge was recorded. Costs for this category are estimated as the weighted average of the costs of the other crime categories.

the net cost per arrest to society while the middle column presents the cost per arrest to the thief.

3. The Resulting Estimates

The estimated values of the crime-related changes generated by Supported Work from the social perspective are presented in Tables III.11 through III.13. As was mentioned, the large reductions in arrests for ex-addicts translated into large social benefits. In particular, the reductions in robbery and burglary arrests combined with the high costs of these crimes lead to resource savings worth over $1,700 per participant during months 1-27. For the other crime types there was a small social cost (resulting from slight increases in larceny). Thus, when all crime types were considered we estimated that Supported Work induced crime reductions among ex-addicts that were worth $1,678 per participant during months 1-27.

For ex-offenders, there was an estimated increase in the social cost of crime of $1,047 per participant during the same period. This is due to increases in the number of burglary and larceny arrests. Because this result is not statistically significant, however, although unbiased, it must be interpreted with caution.

For youth, the results mirror the fact that we found no program effect on the criminal behavior of this group. The estimated social value of the slight decrease in arrests during months 1-27 was about $100 per participant (the reductions in robbery and burglary being offset by the increase in felonious assaults). Again, these results are not statistically significant.

TABLE III.11

PRESENT VALUE OF CRIME RELATED NET SOCIAL BENEFITS OF SUPPORTED WORK, MONTHS 1-27

EX-ADDICT SAMPLE

(dollars)

	Criminal Justice System Benefits	Personal Injury and Property Damage Benefits[a]	Stolen Property Benefits[b]	Total Discounted Benefit
Murder and Felonious Assault	4	7	0	11
Robbery	964	45	38	1,047
Burglary	448	41	176	665
Larceny and Motor Vehicle Theft	-103	-16	-50	-169
Drug Law Violations	196	0	0	196
Other Personal Crimes	-3	0	0	-3
Other Miscellaneous Crimes	-9	0	0	-9
Unspecified Crimes[c]	-48	-4	-8	-60
Total Benefits	1,449	72	156	1,678

NOTE: These figures are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

[a] The drug law violations and other miscellaneous crimes categories contain primarily "victimless" crimes. This implies direct losses to victims are small; hence a value of zero is assumed.

[b] Stolen property social costs, estimated only for property crimes, are estimated as a fraction (65 percent) of the average value of property stolen per arrest.

[c] The unspecified crimes category contains arrests for which no charge was recorded. Costs for this category are estimated as the weighted average of the costs of the other crime categories.

TABLE III.12

PRESENT VALUE OF CRIME RELATED NET SOCIAL BENEFITS OF SUPPORTED WORK, MONTHS 1-27

EX-OFFENDER SAMPLE

(dollars)

	Criminal Justice System Benefits	Personal Injury and Property Damage Benefits[a]	Stolen Property Benefits[b]	Total Discounted Benefit
Murder and Felonious Assault	-54	-97	0	-151
Robbery	-50	-2	-2	-54
Burglary	-179	-16	-70	-265
Larceny and Motor Vehicle Theft	-255	-40	-124	-419
Drug Law Violations	-69	0	0	-69
Other Personal Crimes	26	3	0	29
Other Miscellaneous Crimes	30	0	0	30
Unspecified Crimes[c]	-118	-10	-20	-148
Total Benefits	-670	-162	-216	-1,047

NOTE: These figures are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

[a] The drug law violations and other miscellaneous crimes categories contain primarily "victimless" crimes. This implies direct losses to victims are small; hence a value of zero is assumed.

[b] Stolen property social costs, estimated only for property crimes, are estimated as a fraction (65 percent) of the average value of property stolen per arrest.

[c] The unspecified crimes category contains arrests for which no charge was recorded. Costs for this category are estimated as the weighted average of the costs of the other crime categories.

TABLE III.13

PRESENT VALUE OF CRIME RELATED NET SOCIAL BENEFITS OF SUPPORTED WORK, MONTHS 1-27

YOUTH SAMPLE

(dollars)

	Criminal Justice System Benefits	Personal Injury and Property Damage Benefits[a]	Stolen Property Benefits[b]	Total Discounted Benefit
Murder and Felonious Assault	-97	-174	0	-271
Robbery	421	20	17	458
Burglary	82	7	32	121
Larceny and Motor Vehicle Theft	-70	-11	-34	-115
Drug Law Violations	-78	0	0	-78
Other Personal Crimes	13	2	0	15
Other Miscellaneous Crimes	31	0	0	31
Unspecified Crimes[c]	-47	-4	-8	-59
Total Benefits	256	-160	7	102

NOTE: These figures are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

[a] The drug law violations and other miscellaneous crimes categories contain primarily "victimless" crimes. This implies direction losses to victims are small; hence a value of zero is assumed.

[b] Stolen property social costs, estimated only for property crimes, are estimated as a fraction (65 percent) of the average value of property stolen per arrest.

[c] The unspecified crimes category contains arrests for which no charge was recorded. Costs for this category are estimated as the weighted average of the costs of the other crime categories.

The benefits to nonparticipants are the same as those for society as a whole, except for the changes in stolen property, the distributional consequences of which are shown in Table III.14. The nonparticipant perspective shows the total value of the change in stolen property per participant, the participant column reflects our assumption that participants would convert stolen property into cash at a 35 percent rate, and the social column repeats the totals shown for stolen property in Tables III.11 through III.13.

4. Society's Willingness to Pay for Reductions in Crime

Ideally, by the same logic as was used in the value of output discussion, the value of crime reductions from the social perspective should be measured by the amount society would be willing to pay to bring those reductions about rather than by the resource cost savings achieved (which is the method we have used). Although empirical estimation of social willingness to pay is not feasible, a brief discussion of how it might be measured is useful in interpreting the resource cost savings estimates actually used.

If political and economic processes functioned perfectly, the willingness to pay (i.e., the demand) for crime reductions could be measured by the social expenditures on crime reducing activities. This is so because public and private decision makers would act so that the benefits to be gained from increments in crime reduction activities would equal the cost of generating those increments. Thus, for small changes in crime, the system cost savings brought about by crime reductions would measure both the resource savings from and the demand for the crime reductions. Since, in this case, the system cost savings would equal

TABLE III.14

BENEFITS PER PARTICIPANT OF REDUCED STOLEN PROPERTY MONTHS 1-27, BY ACCOUNTING PERSPECTIVE, BY TARGET GROUP (dollars)

	Perspective		
	Social	Participant	Nonparticipant
Ex-Addicts	156	-84	240
Ex-Offenders	-216	116	-333
Youth	7	-4	11

NOTE: Values are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

willingness to pay, there would be no need for direct estimates of personal injury, property damage, stolen property, or psychological cost savings, because they would be captured in the revealed demand for crime reduction.

The procedure used here may, for this reason, be double counting some of the benefits of crime reductions because it includes values for reductions in personal injury, property damage, and stolen property, in addition to the change in criminal justice system costs. Three considerations, however, suggest that this double counting may be at least partially offset by compensating biases. First, only part of the change in crime prevention costs has been measured since private crime prevention activities have been ignored. Second, equating willingness to pay with the marginal costs of crime prevention may not hold since the technology of crime prevention is imperfectly understood. Third, the social demand for crime reduction may be expressed only imperfectly through the political and economic processes.

Table III.15 shows total crime-related benefits from the social perspective as measured by total resource cost savings (the benchmark estimate) and as measured by criminal justice system cost savings alone. For ex-addicts, nonsystem costs were a relatively small part of total benefits so there is not much difference between estimates. For ex-offenders the difference is also modest, although bigger in percentage terms. In the case of youth the difference is small but in the unexpected direction due to high nonsystem costs associated with their slight estimated increase in murder and felonious assault arrests. By eliminating these costs, social benefits are increased by over $150. However, the magnitudes

TABLE III.15

SOCIAL BENEFIT PER PARTICIPANT OF REDUCED CRIME, MONTHS 1-27, BY TARGET GROUP

(dollars)

	Ex-addicts	Ex-offenders	Youth
Benchmark Estimate[a]	1,677	-1,048	103
Criminal Justice System Costs Only	1,449	-670	256

NOTE: Values are discounted to midpoint of first 9-month period at a 5 percent annual rate.

[a] This includes estimated benefits from net reductions in personal injury and property damage and the social component of stolen property cost reductions.

377

of these differences are small compared to the combined values of all benefits and costs. Therefore, using criminal justice system savings as a proxy for social willingness to pay for crime reductions would not alter our general conclusions.

F. REDUCED ABUSE OF DRUGS AND ALCOHOL

In general, as is shown in detail in the target group reports, Supported Work did not have a pronounced effect on experimentals' drug and alcohol abuse. There were no significant overall changes in drug abuse for ex-addicts or youth, a small reduction in drug abuse for ex-offenders, and a mixture of increases and decreases in alcohol abuse over time for all three of these target groups. (Early results showed no evidence of drug and alcohol abuse among the AFDC target group members, so questions relating to these activities were dropped in later interviews.)

Although no effort was made to measure society's willingness to pay for such changes as were observed,[1] we did value the direct benefit of the resource costs savings from reductions in drug or alcohol treatment. (Indirect benefits that might have resulted are captured in other benefit components such as increased earnings or decreased criminal activity.)

The estimated values of the changes in drug and alcohol treatment are summarized for each target group in Table III.16. The measured effects of Supported Work on the use of treatment programs (control-

---

[1] Because the effects of Supported Work on drug and alcohol abuse were small and varied in sign, the unmeasured benefits coming from satisfying nonparticipant (and possibly even participant) preferences for reductions in these abuses would, in any case, be small.

TABLE III.16

SOCIAL BENEFITS PER PARTICIPANT OF REDUCED DRUG AND ALCOHOL TREATMENT, MONTHS 1-27 (dollars)

	Benefits from Reduced Drug Treatment	Benefits from Reduced Alcohol Treatment	Total Discounted Social Benefit[a]
Ex-Addicts	-4	0	-3
Ex-Offenders	-47	53	6
Youth	-25	-1	-26

NOTE: Values are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

[a] Detail may not sum to total because of rounding

experimental differences in mean number of months in treatment) were valued using the average costs of the major types of drug and alcohol treatment programs--methadone maintenance, residential and nonresidential drug-free treatment programs, inpatient and outpatient detoxification programs, and inpatient, outpatient, and intermediate alcohol treatment programs.[1] As can be seen, the estimated overall values of drug treatment benefits during the 27 months after enrollment are negligible. The net effects are a $6 per participant benefit from decreased treatment costs for ex-offenders, but an increase in treatment costs of $3 per participant for ex-addicts and $26 per participant for youth.

It is important to note that benefits from reduced drug and alcohol abuse treatment may not appear in the short run. Even if Supported Work has the expected long-run behavioral effect of encouraging participants to reduce their use of drugs, the short-run effect could be either a (1) reduction in the need for treatment, or (2) an increase in the desire for treatment in order to reduce drug dependence. When this is considered along with the weak effects Supported Work appears to have had on actual abuse, it is not surprising that the treatment findings are small and not consistently in one direction.

G. USE OF ALTERNATIVE EDUCATION AND EMPLOYMENT SERVICES

In the absence of Supported Work, at least some experimentals would have entered alternative education and training programs. These alternative programs generate a wide variety of benefits and costs in much the same way as Supported Work. Their employment, receipt of transfers, and criminal activity might all have been affected. Thus, the benefit-cost evaluation must be made in relation to the mix of alternative experiences experimentals would have undergone in the absence of Supported Work.

---

[1] These cost estimates are based on published data regarding drug and alcohol treatment. See Bjorklund et al. (1975) and Hertzman and Montague (1977) for discussions of treatment costs.

The evaluation design allows this comparison to be made by using experimental-control differences in behavior. Controls, while being prohibited from entering Supported Work, were allowed to enroll in alternative programs. They therefore represent an alternative treatment group rather than a treatment-free control group; experimental-control differences in behavior can, thus, be attributed to the effectiveness of Supported Work as compared to the mix of alternatives experienced by the control group.

Because the experimental-control differences used to measure Supported Work's effects already include benefits from alternative education and employment programs, it is important to include in the analysis the costs associated with these benefits.

Seven categories of education, training and employment programs were included. In each case, the differences in the mean number of weeks experimentals (relative to controls) used each alternative program were valued by multiplying them by that program's average cost per student week. In addition, differences in registrations with the U.S. Employment Service were valued by the estimated difference in mean number of employment service registrations by the average cost of a registration. The estimates of average cost of the different programs described in more detail in Thornton and Long (forthcoming) are: high school ($46/week), vocational school ($23/week), college and university education ($85/week), unspecified schooling ($51/week),[1] WIN training ($74/week), CETA and other training ($38/week),[2] public service employment ($14/week), and U.S. Employment Service ($43/registration). These estimates exclude allowances or wage payments to participants.

The results are shown in Table III.17. As can be seen, the effect of Supported Work on experimentals' use of alternative education and employment programs in months 1-27 was a small relative overall reduction for all target groups. None of the differences in average number of weeks enrolled in any of the specific programs considered here was greater than two weeks. (AFDC experimentals did, however, spend slightly more time in school programs than controls, although the differences are not statistically significant.) The estimated value of the resources saved because of these overall reductions ranged from $72 per participant for the ex-addict target group to $136 per participant for the ex-offender group. This is a benefit from both the nonparticipant and the social perspectives.

We also estimated the experimental-control differences in training allowances (from programs other than Supported Work). A reduction in payments is a cost to participants, a benefit to nonparticipants, and does not therefore enter the social benefit-cost calculations. The estimates, presented in Table III.18, indicate that experimentals in the

---

[1] When respondents reported being in school, but did not report the type, it was categorized as unspecified schooling and the value used was the average of the costs of the other schooling types.

[2] Since CETA funded programs represent the vast majority of training programs available to the Supported Work target groups, all non-WIN training programs were treated together using the average costs of CETA Title I programs.

TABLE III.17

BENEFIT PER PARTICIPANT OF REDUCED USE OF ALTERNATIVE EDUCATION, TRAINING, AND EMPLOYMENT PROGRAMS, MONTHS 1-27

(dollars)

	Reduced Use of School	Reduced Use of Training and Employment Programs	Total Discounted Value[a]
AFDC	-27	161	133
Ex-Addicts	48	23	72
Ex-Offenders	75	62	136
Youth	61	26	87

NOTE: Values are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

[a] Details may not sum to totals because of rounding.

TABLE III.18

BENEFIT PER PARTICIPANT OF REDUCED TRAINING ALLOWANCES FROM ALTERNATIVE PROGRAMS PAID TO EXPERIMENTALS, MONTHS 1-27 (dollars)

Target Group	Total Discounted Value of Change in Allowances
AFDC	-10
Ex-Addict	13
Ex-Offenders	32
Youth	-4

NOTE: Values are discounted to the midpoint of the first 9-month period at a 5 percent annual rate.

AFDC and youth groups received more income from training allowances than did controls, while ex-addict and ex-offender experimentals received less. However, all differences are small.

A caveat is necessary with respect to the interpretation of these results. If the evaluation period were long enough for all the possible future effects of Supported Work and the alternative programs to be reflected in the experimental-control differentials used to estimate those effects, our procedure would lead to unbiased results. Because the evaluation period is finite, however, we cannot be sure there is no bias.

If, for example, the short evaluation period meant that controls did not have time to complete the education and employment alternatives, observed control earnings may understate the earnings to be expected in the future and lead the experimental-control differences, thus, to be overstated. If, in contrast, Supported Work motivates participants to obtain additional education and employment services they would not have obtained if it had not existed, the finite nature of the observation period will lead, by analogous reasoning, to the long-run earnings of experimentals being understated and the effects of Supported Work on earnings (i.e., the experimental-control difference), consequently , understated as well.

One way to avoid these biases in such a short-run evaluation would be to incorporate estimates of the returns to investments in human capital into the evaluation.[1] In this way the present value of any future earnings gains associated with participation in alternative programs could be figured into our estimates of the Supported Work effect on long-run earnings. For several reasons this method was not adopted. First, the inclusion of these estimates would lead to the double-counting of some earnings gains, unless the effects of such human capital investments could be eliminated from our direct measures of earnings based on interview data. Second, many of the studies that estimate returns to education deal with populations that are not nearly as disadvantaged as the Supported Work target groups; therefore, their estimates may be inappropriate for this evaluation. Finally, it is not at all clear that the returns to completing an education or training program can be used to infer the returns to completing a portion of a program.[1] Even for those cases in which returns to a year of schooling have been estimated, these estimates are inappropriate for valuing the changes of a few months of discontinuous participation.

---

[1] Such estimates for schooling are contained in Jacob Mincer (1974) and Zvi Griliches (1978).

---

[1] There is substantial evidence of a "diploma effect," in which the benefits from completing a program are significantly higher than those gained by almost completing the program.

CHAPTER IV

OVERALL RESULTS FOR THE YOUTH TARGET GROUP

As in the case of the AFDC and ex-addict target groups, the benefits of Supported Work exceeded its costs during months 1-27 from the youth participant perspective. Unlike the results for the other two groups, however, from both the social and nonparticipant perspectives benefits fell short of the costs for the youth group. As Table IV.1 shows, this is because the only benefit to youth came from their in-program earnings (about $3,600 per participant),[1] which exceeded their costs (foregone earnings, increased taxes, and reduced welfare payments) by about $1,800.[2] From the social perspective benefits were very small and were far outweighed by costs. Social costs totaled $5,000 per participant, with the only substantial benefit to offset it from the social perspective being in-program output valued at $3,400 per participant. Other benefits totaled only about $200 per participant resulting in a negative net present value from the social perspective of $1,377. From the perspective of nonparticipants, the situation was even worse, with benefits falling short of costs by almost $3,200.

The pattern of benefits and costs over time (shown in Table IV.2) was similar to that for the AFDC and ex-addict target groups. Social

---

[1] As can be seen in Chapter III, part of the reduction in transfer payments is in payments to other household members (presumably participants' parents). This is treated as a cost to participants although it is technically a cost to their families.

[2] Youths also had a small estimated increase in training allowances, and small measured reductions in post-program earnings and stolen property.

Rev. Shell: 10/26/79

TABLE IV.1

MEASURED BENEFITS AND COSTS PER PARTICIPANT, MONTHS 1-27, BY ACCOUNTING PERSPECTIVE

YOUTH SAMPLE

(dollars)

	Accounting Perspective		
	Social	Participant	Nonparticipant
Benefits			
I. Output Produced by Participants			
• Value of in-program output	3,394	0	3,394
• Increased post-program earnings plus fringes	-3	-3	0
II. Increased Tax Payments	0	-301	301
III. Reduced Dependence on Transfer Programs			
• Reduced transfer payments	0	-474	474
• Reduced administrative costs	78	0	78
IV. Reduced Criminal Activity			
• Reduced property damage and personal injury	-160	0	-160
• Reduced stolen property	7	-4	11
• Reduced justice system costs	256	0	256
V. Reduced Drug and Alcohol Treatment Costs	-26	0	-26
VI. Reduced Use of Alternative Education, Training, and Employment			
• Reduced education and employment costs	87	0	87
• Reduced training allowances	0	4	-4
Costs			
I. Program Operation Costs			
• Project costs	-2,136	0	-2,136
• Overhead costs	-1,697	0	-1,697
II. Central Administrative Costs	-203	0	-203
III. Participant Labor Costs			
• In-program earnings plus fringes	0	3,551	-3,551
• Foregone earnings plus fringes	-974	-974	0
Net Present Value (Benefits minus Costs)	-1,377	1,800	-3,177

NOTE: Components have been arranged and estimated values entered according to a priori expectations about whether they would be benefits or costs from the social and nonparticipant perspectives. This arrangement is reflected in the accounting framework presented in Table I.1. Positive values (benefits) and negative values (costs) in this table represent actual outcomes rather than the expectations given in Table I.1.

TABLE IV.2

BENEFITS AND COSTS FROM THE SOCIAL AND PARTICIPANT PERSPECTIVES, BY TIME PERIOD

YOUTH SAMPLE

	Months 1-9	Months 10-18	Months 19-27
Social Perspective			
Benefits			
Value of in-program output	2,659	721	43
Increased post-program earnings plus fringe benefits	58	-126	65
Reduced administrative cost of transfer programs	45	18	17
Reduced cost of criminal activity[a]	59	-112	165
Reduced cost of alternative education and training programs	89	8	-11
Reduced drug and alcohol treatment costs	-6	-4	-18
Costs			
Program operating and central administrative costs[b]	-3,163	-857	-51
Foregone earnings plus fringe benefits	-700	-265	-20
Net Present Value (Benefits minus Costs)	-957	-619	190
Participant Perspective			
Benefits			
In-program earnings plus fringe benefits	2,770	754	58
Increased post-program earnings plus fringe benefits	58	-126	65
Costs			
Foregone earnings plus fringe benefits	-700	-265	-20
Increased tax payments	-265	-28	-9
Reduced transfer payments	-329	-102	-51
Reduced training allowances	-14	-16	35
Reduced stolen property	5	30	-41
Net Present Value (Benefits minus Costs)	1,526	248	38

NOTE: To facilitate comparisons over time, benefits and costs have been discounted. A positive number is a benefit; a negative number is a cost.

[a] This is the sum of reduced property damage and personal injury, stolen property, and criminal justice system costs.

[b] This is the sum of project, overhead, and central administrative costs.

benefits fell most short of costs during months 1-9, a largely in-program period, and they continued to fall short during months 10-18, primarily because estimated value of in-program output did not cover costs. Unless there are substantial unmeasured benefits during the in-program period--and, given society's preference to have youth working, there could be--whether overall net present value is positive from society's perspective for youth depends on future benefits. From the participant perspective, the in-program period is when their net incomes increase the most, largely because of Supported Work earnings; participant benefits still exceed costs in months 19-27, but only by $38 per participant.

From the nonparticipant perspective (not shown), the situation is, again, the worst during months 1-9, when nonparticipant benefits fell short of costs by about $2,500; after this largely in-program period, the nonparticipant results improved but were barely positive by months 19-27.

Table IV.3 shows the alternative base period estimates for extrapolation of the youth results. There is little evidence of benefits beyond the 27th month. The 36-month cohort results show negative benefits for post-program earnings and criminal activity during this period, although both could be the result of chance sampling variability. For this target group, too, the 36-month youth cohort was small (the smallest of all target groups)--153 observations--reducing the reliability of the 28-to-36-month results. But, in any case, the estimates of program impacts in this later period provide no indication of long-term benefits for youth: when the benchmark cohort adjustment is used, estimated social benefits in months 28-36 are close to zero (-$15).

TABLE IV.3

MEASURED SOCIAL BENEFITS, ALTERNATIVE BASE PERIOD ESTIMATES

YOUTH SAMPLE

Benefit	Months 19-27	Months 28-36	Benchmark Estimate
Increased Post-Program Earnings plus Fringe Benefits	65	-346	5
Reduced Administrative Costs of Transfer Programs	17	10	26
Reduced Cost of Criminal Activity	165	-169	-34
Reduced Drug Treatment Costs	-18	0	-15
Reduced Education and Employment Costs	-11	38	2
Total	218	-466	-15

NOTE: The results given in the table for months 19-27 are for the full 27-month sample (which includes the 27-month and the 36-month cohorts); those for months 28-36 are for the 36-month cohort; the benchmark estimate is the weighted average of (1) the 19-to-27-month results for the 27-month cohort and (2) the 28-to-36-month results for the 36-month cohort.

Table IV.4 presents the benchmark estimates from all three perspectives. Given that benefits after month 27 are essentially zero, these results differ little from those based on data for the first 27 months only.

These conclusions are also remarkably insensitive to alternative assumptions. This is apparent in Table IV.5, which presents the sensitivity tests for the youth results. Only if participant labor from society's perspective is assumed to be free (i.e., the shortage labor market model), does net present value become positive from the social and nonparticipant perspectives; with minor exceptions, it is always positive from the participant perspective.[1]

Thus, we conclude that, based only on the measured benefits and costs, the results for youth are clear-cut. Benefits fell short of costs from the social and nonparticipant perspectives, participants were made better off, but only as a result of their Supported Work earnings.

How one interprets these results depends on how highly unmeasured benefits are valued. One of the original hopes for Supported Work was that it would encourage youth participants to return to school, but Maynard (1979) finds no evidence that this hope was realized. Neither was there any evidence of improved health status or reduced drug use.

Supported Work did, however, put the youth participants to work during the in-program period, and judging from the large number of youth

---

[1] As for the AFDC target group, the few negative estimates from the participant perspective arise because a sizable estimated reduction in transfer payments during the base period is extrapolated into the future. It seems unlikely that such a reduction in transfer payments would persist far into the future, however, given the other indications of future behavior on the part of this group.

TABLE IV.4

BENCHMARK ESTIMATES OF BENEFITS AND COSTS PER PARTICIPANT, BY ACCOUNTING PERSPECTIVE

YOUTH SAMPLE

(dollars)

	Perspective		
	Social	Participant	Nonparticipant
Benefits			
I. Output Produced by Participants			
• Value of in-program output	3,394	0	3,394
• Increased post-program earnings plus fringes	29	29	0
• Preferences for work over welfare	a/	a/	a/
II. Increased Tax Payments	0	-341	341
III. Reduced Dependence on Transfer Programs			
• Reduced transfer payments	0	-1,361	1,361
• Reduced administrative costs	228	0	228
IV. Reduced Criminal Activity			
• Reduced property damage and personal injury	-1,346	0	-1,346
• Reduced stolen property	404	-218	622
• Reduced justice system costs	853	0	853
• Reduced psychological costs	a/	a/	a/
V. Reduced Drug and Alcohol Use			
• Reduced treatment costs	-116	0	-116
• Psychological benefits	a/	a/	a/
VI. Reduced Use of Alternative Education, Training, and Employment Services			
• Reduced education and employment costs	100	0	100
• Reduced training allowances	0	205	-205
VII. Other Benefits			
• Improved participant health status	a/	a/	a/
• Income redistribution	a/	a/	a/
Costs			
I. Program Operating Costs			
• Project costs	-2,136	0	-2,136
• Overhead costs	-1,697	0	-1,697
II. Central Administrative Costs	-203	0	-203
III. Participant Labor Costs			
• In-program earnings plus fringes	0	3,551	-3,551
• Foregone earnings plus fringes	-974	-974	0
• Foregone leisure	a/	a/	a/
IV. Increased Work Related Costs			
• Child care	0	0	0
• Other	a/	a/	a/
Net Present Value (Benefits minus Costs)	-1,465	892	-2,357

NOTE: As before, the components are listed according to their expected status as benefits or costs from the social perspective. Whether the data showed them to be net benefits or net costs or neither is indicated by (+) or (-), respectively.

a/ These benefits and costs were not measured.

10/25/79

TABLE IV.5

NET PRESENT VALUE ESTIMATES UNDER ALTERNATIVE ASSUMPTIONS

YOUTH SAMPLE

	Perspective		
	Social	Participant	Nonparticipant
Benchmark Estimates	-1,465	892	-2,357
Extrapolation Assumptions			
Discount rate = 3 percent	-1,481 (-16)	768 (-124)	-2,249 (+108)
Discount rate = 10 percent	-1,434 (+31)	1,114 (+222)	-2,548 (-191)
Time Horizon: No benefits beyond 27 months	-1,377 (+88)	1,800 (+908)	-3,177 (-820)
Time Horizon: No benefits beyond 36 months	-1,390 (+75)	1,661 (+769)	-3,052 (-695)
Decay Rate = 3 percent	-1,597 (-132)	-478 (-1,370)	-1,119 (+1,238)
Base = 19-27 month results	-250 (+1,215)	1,799 (+907)	-2,049 (+308)
Base = 36 month cohort results (without adjustment)	-4,118 (-2,653)	-379 (-1,271)	-3,739 (-1,382)
Employment Assumptions			
Shift from surplus to shortage market	17,999 (+19,464)	892 (+0)	17,107 (+19,464)
Shuffling workers among jobs	-1,777 (-312)	892 (+0)	-2,669 (-312)
CETA-WIN PSE wages valued at 50 percent	-1,566 (-101)	892 (+0)	-2,458 (-101)
Cost and Value of Output Assumptions			
Value of output equals revenue	-2,999 (-1,534)	892 (+0)	-3,891 (-1,534)
Inclusion of start-up costs	-2,425 (-960)	892 (+0)	-3,317 (-960)
Crime Assumptions			
No crime benefits	-1,376 (+89)	1,109 (+217)	-2,485 (-124)
50 percent of crime benefits	-1,420 (+45)	1,001 (+109)	-2,421 (-64)
Criminal justice system costs only	-523 (+942)	1,109 (+217)	-1,633 (+724)

NOTE: Numbers in parentheses show the amount and direction of change from the appropriate benchmark estimate given to the top line of the table.

employment program initiatives, this seems to be something society desires to do. Indeed, providing youths with initial work experience, as mentioned in Chapter I, has long been an objective of employment programs for youth. In addition, Supported Work has redistributed a modest amount of income to the youth, another unmeasured benefit of Supported Work, although this has been accomplished at considerable cost. The overall assessment of Supported Work for the youth target group, thus, depends on how much society, and particularly nonparticipants, are willing to pay to put youth to work and redistribute income from the rest of society to them. If Supported Work for youth is to be continued, the absence of substantial benefits other than value of output suggests the need to search for less costly jobs with higher value of output.[1]

---

[1] Maynard (1979) does find (admittedly weak) evidence that younger members of this group (who in the absence of Supported Work are most likely to be unemployed) seem to do better, suggesting that it may also be desirable to attempt to target the program at younger applicants.

CHAPTER V

COMPARISON TO OTHER BENEFIT-COST STUDIES OF EMPLOYMENT AND TRAINING PROGRAMS

Benefit-cost analysis has been applied to a number of employment and training programs over the past twenty years. This chapter attempts to compare the results of these studies with the results presented for Supported Work in this report. It must be stressed at the outset that comparisons among programs are difficult to make and, when made, susceptible to misinterpretation because of inevitable differences among target groups served, prevailing economic conditions, and analytical approaches used in the evaluations. Nay, Scanlon, and Wholey (1973) struggled with such differences in an earlier comparison of benefit-cost results for employment and training programs,[1] and concluded that "interpretations and comparisons of benefit-cost ratios based on the numbers alone, without regard to the particular viewpoints and definitions behind each of the numbers used, are almost certain to be misleading." Although these difficulties make any strict comparison of Supported Work relative to all other employment and training programs impossible, useful research and policy implications can be drawn from an examination of other benefit-cost research.

After a general overview of major benefit-cost analyses undertaken since 1960, evaluations of four programs serving target groups that are similar to those served by Supported Work will be examined in detail.

---

[1] Stromsdorfer (1972) and "Cost-Benefit Analysis and the Job Corps" (1979) also mention these difficulties in their comparisons of benefit-cost analysis.

A. OVERVIEW OF PREVIOUS BENEFIT-COST STUDIES

Table V.1 presents descriptive data and results of the major benefit-cost analyses of employment and training programs that have been conducted since 1960. Looking at the last set of columns of Table V~~II~~I.1, we can see that the variation in results is quite striking. The benefit-cost ratios presented range from less than zero to over 100, implying a wide variation in the measured cost-effectiveness of different programs. As can also be seen from the table, the programs varied widely in target groups served, urban/rural orientation, and macroeconomic conditions. In addition, the methodological differences from study to study are so important in some cases that comparing the benefit-cost ratios of the studies is meaningless. These differences are of three general types: (1) the quality of the outcome measures (comparison group and statistical methodology and sample size); (2) the accounting framework (benefits and costs included and perspectives taken); and (3) extrapolation (discount rates, decay rate, and time horizon). Each of these types of differences will be discussed, along with the implications for comparisons among the studies.

As in the Supported Work evaluation, outcomes are typically measured by computing differences for variables such as earnings between participants and a relevant comparison group. The ideal way to measure these differences is to use double blind random assignment of individuals

Rev. 10/26/79

TABLE V.1

A COMPARISON OF BENEFIT-COST STUDIES OF EMPLOYMENT AND TRAINING PROGRAMS: DESCRIPTIVE DATA AND RESULTS

Program (Evaluation in Parentheses)	Target Group	Services Provided	Time Period of Study	Locus of Study	Benefit-Cost Ratios (Net Present Value per Participant in Parentheses)
Area Redevelopment Act (ARA) and state government program (Cain and Stromsdorfer, 1968)	Unemployed and underemployed workers in depressed areas	Classroom instruction	1961-62	West Virginia	Male / Female / All 5% discount rate: 16.5 ($14,200) / 3.3 ($1,234) / 14.1 ($10,136) 10% discount rate: 10.5 ($8,732) / 2.9 ($981) / 9.3 ($6,553)
ARA and state government program (Stromsdorfer, 1968)	Unemployed and underemployed workers in depressed areas	Classroom instruction	1959-63	West Virginia	Male / Female 4% discount rate: 15.6 ($13,382) / 12.0 ($5,773) 6% discount rate: 14.6 ($12,482) / 9.3 ($4,373)
ARA and Manpower Development and Training Act (MDTA) (Hardin and Borus, 1972)	Unemployed and underemployed workers	Classroom instruction	1962-65	Michigan	Hours of Training 60-200: 17.34 ($5,654) 201-600: -0.03 ($-912) 601-1,200: -0.34 ($-2,925) 1,201-1,920: -0.25 ($-4,116) (Assumptions of 10% discount rate and 10 year duration of benefits were used.)
MDTA (Borus, 1964)	Experienced members in work force suffering high unemployment	Classroom instruction OJT	1962-63	Connecticut	18% flow per year out of occupations which use training: 73.3 ($20,869) 18% flow per year for first three years, then 0% flow: 103.8 ($29,677)
MDTA (Sewell, 1971)	Rural clientele, mostly black, male, heads of families; many farm workers or underemployed	Classroom instruction, OJT	1965-66	North Carolina	Training: OTJ Females 6.8 ($6,926) / OTJ Males 3.3 ($2,781) / Institutional Males 1.7 ($1,866)

Table V.1 (continued)

Program (Evaluation in Parentheses)	Target Group	Services Provided	Time Period of Study	Locus of Study	Benefit-Cost Ratios (Net Present Value per Participant in Parentheses)
Neighborhood Youth Corps (NYC) In-School and Summer Programs (Somers and Stromsdorfer, 1970)	Youths--14 years old and older	Job market orientation, work experience	1965-67	Nationwide	In-School: ($384) Summer: ($126) Both In-School and Summer: ($462) (Benefits assumed to last 17, 21, and 18 months respectively for the three categories listed.)
NYC Out-of-School Program (Borus, Brennan, and Rosen, 1970)	Youths--16 years old and older	Skill training, placement	1967	Indiana	NYC Output Excluded — Male / Female; NYC Output Included — Male / Female Foregone earnings: 2.8 / .4; 3.3 / 1.0 No foregone earnings: 6.1 / .9; 7.4 / 2.1 (Results are for those with 10 years of education.)
Job Corps (Cain, 1967)	Disadvantaged men and women ages 16 to 21 who are out of work and lack marketable job skills	Basic education, high school equivalency instruction, skill training, work experience, placement	1966	Nationwide	Educational Gains / Wage Gains 3% discount rate: 1.2[illegible] ($780) / 1.45 ($1,616) 5% discount rate: .79 ($-740) / 1.04 ($158)
Job Corps (Thornton, Long, and Mallar, 1978)	Same as above	Same as above	1977	Nationwide	Discount Rate: 3% 1.15 ($757); 5% 1.05 ($251); 10% .88 ($-582) (Assumption of 14% benefit decay rate per year was used.)
WIN II (Schiller, 1978)	AFDC recipients	Job search, education, training, subsidized employment	1974-76	Nationwide	Males / Females 3% discount rate: 2.11 ($841) / 1.35 ($382) 6% discount rate: 1.74 ($561) / 1.18 ($204) (Assumption of 19% benefit decay rate per year was used.)

Table V.1 (continued)

Program (Evaluation in Parentheses)	Target Group	Services Provided	Time Period of Study	Locus of Study	Benefit-Cost Ratios (Net Present Value per Participant in Parentheses)
Living Insurance for Ex-offenders (LIFE) (Mallar and Thornton, 1978)	Males released from Maryland prisons who had high probability of committing theft crime	Transfers	1972-74	Baltimore, Maryland	Lower Bound 4.0 Upper Bound 53.7 (5% discount rate, no future crime benefits, and no decay for other benefits assumed for upper bound estimate; 15% discount rate, negative future crime benefits and 14% decay rate for other benefits assumed for lower bound estimate.)
Wildcat (Friedman, 1977)	Ex-addicts with record of unemployment	Temporary employment	1972-74	New York City	1.64 ($2,207) (Benefits and costs cover first year following enrollment.)
Wildcat (Friedman, 1978)	Ex-addicts with record of unemployment	Temporary employment	1972-76	New York City	1.12 ($1,678) (Benefits and costs cover first 3 years following enrollment.)
National Supported Work Demonstration (Kemper, Long, and Thornton, 1979)	AFDC, ex-addicts, ex-offenders, youth	Temporary employment	1975-78	Nationwide (10 cities)	AFDC 2.17 ($8,150) Ex-addicts 1.83 ($4,345) Ex-offenders -- Youth .71 ($-1,465) (Assumption of 5% discount rate, 3% benefit decay rate for AFDC and 17% decay rate for other groups were used for benchmark estimates.)

to experimental and control groups,[1] so that the measured differences between the two groups are controlled for and unbiased estimates of the impacts of the program can be obtained. When such a formal experimental design is not possible, a comparison group must be chosen in a way that minimizes sources of potential bias. Any differences between the two groups for which the statistical estimation procedures do not control may impart biases of unknown direction and magnitude to the measured effects of the program.

Table V.2 summarizes the methodologies used in these studies. Of the analyses covered, only LIFE used double blind random assignment. Three additional studies--the Supported Work study and the two Wildcat studies[2]--used random assignment although the experimentals and controls knew to which group they belonged. The other analyses in the table used comparison groups of varying quality--some used early dropouts, some used applicants who did not enroll, and others used nonapplicants who were similar to the participants. Of those studies that did not use random assignment, all but two [3] used statistical techniques (typically multivariate regression) to control for any differences between the treatment and

---

[1] In a double blind experiment program operators do not know in advance to which treatment group a person will be assigned, and experimentals and controls do not know to which group they themselves have been assigned. When controls know that they have been rejected from a program because it is an experiment, and experimentals know they are participating in a program that is being evaluated, the knowledge may affect their behavior in unknown ways. A double blind experimental design avoids these biases.

[2] The data used for the Wildcat studies had an additional shortcoming: those in the experimental group who did not participate in Wildcat were not interviewed. Dropping these experimentals may have introduced additional unknown biases.

[3] See Cain and Stromsdorfer (1968) and Cain (1967).

TABLE V.2

A COMPARISON OF BENEFIT-COST STUDIES OF EMPLOYMENT AND TRAINING PROGRAMS: METHODOLOGY

Program	Comparison Group Methodology	Sample Size	Benefits	Costs	Discount Rate	Duration of Benefits	Comments
ARA and state government program (Cain and Stromsdorfer, 1968)	Program graduates compared with nonapplicants (unemployed persons from employment service files)	341 experimental 284 comparison	Increased earnings	Program costs Foregone earnings Increase in transfer payments	3% 5% 8% 10%	to age 65	
ARA and state government program (Stromsdorfer, 1968)	Same as above	879 total 333 experimental	Increased earnings	Same as above	4% 6%	to age 65	
ARA and MDTA (Hardin and Borus, 1972)	Program participant with those who applied and accepted but did not enroll	503 experimental 281 comparison	Increased earnings	Program costs Foregone earnings Administrative costs	5% 10% 15%	5-10-15 years	
MDTA (Borus, 1964)	No comparison group used--see comment	373 experimental	Increased earnings including multiplier effect (assuming a multiplier of 2)	Program costs excluding transfers and use of buildings Administrative costs	5%	10 years	Assumed zero opportunity cost of labor. Benefits average total earnings of those who used their training multiplied by the probability of using the training after completion of the course. Borus made alternate assumptions as to the flow out of these occupations over the 10-year benefit period.

Table V.2 (continued)

Program	Comparison Group Methodology	Sample Size	Benefits	Costs	Discount Rate	Duration of Benefits	Comments
MDTA (Sewell, 1971)	Program participants compared with eligible persons who did not participate	362 experimental 182 comparison	Increased earnings	Program costs Foregone earnings Administrative costs	8% 10% 12%	to age 65 adjusting for mortality rates	
NYC--In-School and Summer Programs (Somers and Stromsdorfer, 1970)	Program participants compared with students who met eligibility requirements but did not participate	676 total	Increased earnings	Program costs Foregone earnings	10%	(17, 16, and 21 months, see Table VIII.1)	
NYC--Out-of-School Program (Borus, Brennan, and Rosen, 1970)	Program participants compared with eligible applicants who 1) were not selected, 2) could not be notified of selection, or 3) did not show up	604 experimental 166 comparison	Increased earnings In-program output	Program costs Foregone earnings	10%	10 years	Value of in-program output either assumed to be 0 or equal to program wages. Foregone earnings either assumed to be 0 or equal to program wages.
Job Corps (Cain, 1967)	Program participants compared with applicants who were selected but did not participate	868 experimental 517 comparison	Increased earnings	Program costs excluding transfers Foregone earnings Administrative costs	3% 5% 10%	to age 65	Adjustment made for real growth of earnings. 3% discount rate used is equivalent to a 5% discount rate with 2% growth. Two alternative measures of earnings gains used: one uses measured educational gains from program, second compares wages to those of comparison group.

Table V.2 (continued)

Program	Comparison Group Methodology	Sample Size	Benefits	Costs	Discount Rate	Duration of Benefits	Comments
Job Corps (Thornton, Long, Mallar, 1978)	Program participants compared with persons from dropout files and local unemployment service files	2419 experimental 1321 comparison	Increased earnings In-program output Reduction in administrative costs of transfers Reduced crime Reduced use of alternative training programs	Program costs excluding transfers Foregone earnings Administrative costs	3% 5% 10%	14% decay per year for expected work life (age 61)	Estimates incorporate a 2% per year real growth rate of wages.
WIN II (Schiller, 1978)	Program participants compared with nonparticipants eligible for WIN but not served due to budget constraints	3086 experimental 3523 comparison	Increased earnings	Employment services Related supportive social services	3% 6%	10% 19% 35% decay per year	OJT and PSE wages were counted as part of increased earnings for some of those who were still in subsidized positions at the time of the follow-up interview.
LIFE (Mallar and Thornton, 1978b)	Random assignment to experimental and control groups	216 experimental 216 control	Reduced costs of crime Increased earnings Reduced use of other programs Reduced administrative costs of transfer programs	Administrative costs	5% 15%	40 years with 15% fadeout or 40 years with 0% fadeout	Lower bound estimates were obtained assuming a 15% discount rate, that earnings and welfare rates declined over time at a rate of 14% per year that arrest differentials decreased over time and that the earnings differential included 50% displacement. Upper bound estimates assumed a 5% discount rate, no displacement, and no fadeout. Administrative costs were estimated based on those of unemployment and public assistance programs.
Wildcat (Friedman, 1977)	Same as above	148 experimental 160 control	In-program output Increased post-program earnings Reduced costs of crime Improved health	Program costs minus transfers Foregone earnings	0%	1 year	

Table V.2 (continued)

Program	Comparison Group Methodology	Sample Size	Benefits	Costs	Discount Rate	Duration of Benefits	Comments
Wildcat (Friedman, 1978)	Random assignment to experimental and control groups	194 experimental 207 control	In-program output Reduced costs of crime Reduced transfer payments Increased taxes of participants	Program costs	0%	3 years	Calculations are done from the perspective of the taxpayer
National Supported Work Demonstration (Kemper, Long, and Thornton, 1979)	Same as above	AFDC: 1351 Ex-addicts: 974 Ex-offenders: 1479 Youth: 861	In-program output Increased post-program earnings Reduced costs of crime Reduced welfare administrative costs Reduced drug treatment costs Reduced use of alternative E & T programs	Program costs minus transfers Foregone earnings Administrative costs Increased cost of childcare	5%	14% decay per year for expected working life (age 61)	

comparison groups that were not due to the treatment. No study can control completely for all such differences--to do so, one would have to know the exact relationship between individual characteristics and program outcomes. The outcomes measured will always, therefore, be subject to some uncertainty.

The differences in comparison group methodology among the studies make it particularly difficult to compare the results of different programs. If only those programs which used identical designs (e.g., random assignment) were compared, one would have some confidence that the measured differences among studies were not due differences in the outcome measurement methodology. When comparisons are made between programs with different designs, however, one cannot separate the true differences in program effects from those due to comparison group biases.

A second difference among the studies is the size of the sample used to measure participant effects. The larger the sample size (for a given population variance), the greater the confidence that can be placed in the estimate of a given effect. The sample sizes of the programs listed in Table VIII.2 range from 373 to 6,609 but only two had samples of over 3,000 (Thornton, Long, and Mallar, 1978 and Schiller, 1978).[1/] While determining whether a sample is "big enough" depends on specific evaluation objectives, a study with the same objectives but

---

[1/] This evaluation of Supported Work has a combined sample of over 3,000. Since the four target groups have been analyzed separately instead of pooled together as originally designed, the sample sizes for the specific target group estimates are not large.

a smaller sample size should be viewed as yielding estimates of the same effects which are less precise than those of studies with larger sample sizes.

Sample size is related to the comparison group methodology in the following sense: an ideal comparison group methodology assures that estimates of program impacts are unbiased whatever the size of the sample. The larger the sample, however, the more precise this unbiased estimate will be as a measure of the true effect. Good estimates of a program's effectiveness require large sample sizes and an adequate comparison group methodology. Few studies have both.

The measurement procedure is not the only way in which the studies differ, nor is it the only factor which makes the comparison of programs difficult. The accounting framework--the benefits and costs that are chosen for measurement and the perspectives from which they are viewed--also differs among studies.

Most of the studies were done from the social perspective. One clear exception is the study done by the Vera Institute of Justice (Friedman, 1978) which presented results from the perspective of nonparticipants. Other studies used a mixed perspective. Cain and Stromsdorfer (1968) and Stromsdorfer (1968), for instance, counted a reduction in transfer payments as a benefit[1/] and, Schiller (1978) treats program wages and allowances as a cost[2/], even though the remainder of all three analyses used a social perspective.

[1/] This is inappropriate for the social perspective, as we have noted, because although transfer payment reductions are benefits to nonparticipants they represent costs to participants and thus should not appropriately appear in the social accounting.

[2/] This is inappropriate for the social perspective also, because although they are costs to nonparticipants, they are benefits to participants.

With respect to differences in which benefits and costs are included, authors sometimes disagree about which benefits or costs are small enough to ignore; more often certain benefits and costs have not been measured because the studies lacked the resources or the data to do so. Whatever the reasons, the comparison problem is further aggravated by the differences in benefits and costs considered. If we wanted to compare Supported Work with Cain's (1967) Job Corps study, for example, it would be difficult to know how to treat those benefits (e.g., reduced crime) that are measured for Supported Work but not for the Cain Job Corps study. Dropping the additional Supported Work benefit, recalculating net present value, and comparing the results to that of Job Corps would not necessarily give a correct ranking of the two programs because the excluded benefits might be higher for one program than the other.

A similar problem arises if we want to compare evaluations done from different perspectives--for example, Supported Work and one of the "mixed perspective" studies mentioned earlier. If all the information existed and were readily available, the mixed perspective could be changed to a social perspective by dropping out transfers. While this is conceptually easy to do, however, it is often practically impossible because transfers may not be identified separately and isolated from program costs.

Another aspect of methodology that varies across analyses is the procedure used to extrapolate benefits into the future. The discount rates employed in the studies range from 0 to 15 percent. Assumptions regarding the duration of benefits also vary. Some studies assume a time horizon of one year, others 10 years, and still others the remaining work life of participants (i.e., through age 62 or 65). Some assume that benefits remain in full force this entire time. Others assume that they decay at rates ranging from 0 to 35 percent per year. Differences in extrapolation assumptions do not necessarily present a problem for a comparison, since studies often present results using several different assumptions. If they do not, new benefit-cost results can be calculated so long as the needed information is reported.

Methodological differences, then, are of different types, and not all differences bear equally on the comparability of results. Extrapolation assumptions are easiest to standardize, although the data necessary for such a recalculation are not always published. Differences in accounting frameworks are more difficult to deal with. When perspectives are mixed, it is often impossible to reconstruct estimates from a consistent perspective because the necessary disaggregation of the component results is not reported. When different benefits and costs are measured, there is often no way of making a meaningful comparison of overall results. Finally, differences in the comparison-group methodology used are the hardest of all to deal with since there is no way to standardize across studies, and the effects of biases from inadequate comparison groups or different statistical estimation techniques are unknown.

In spite of these problems, the Supported Work results will be compared in more detail with four other studies--the WIN-II evaluation, the 1978 Job Corps study, the study of LIFE, and the Vera Institute Analysis of Wildcat. Although these studies vary in their degree of comparability with Supported Work, they share certain characteristics which make comparison efforts more likely to be fruitful: they are all recent studies and they had target groups similar to those of Supported

Work. Generally speaking, they also measured similar benefits and costs. Two of them (LIFE and Wildcat) used random assignment to experimental and control groups while a third (Job Corps) made an unusual effort to find a comparison group similar to the experimental group and to control statistically for measured differences.

B. WIN-II

The Work Incentive (WIN) program, established under the Social Security Act in 1968, provides employment, training, and supporting services to welfare recipients. WIN-II (this is the WIN program as reformed in 1972) supplies varying levels of service to recipients, with some receiving minimal services, some receiving placement help or training services, and others receiving partially or fully subsidized employment.

Schiller's (1978) evaluation of WIN-II differs from the Supported Work evaluation in two important respects. First, it relies on a comparison group rather than a randomly selected control group to measure program effects. As noted above, this inevitably creates some uncertainties about the measurement of WIN's effects. Second, the benefit-cost analysis uses a mixed-perspective accounting framework in including participant wages and allowances (instead of foregone earnings) in costs, and it does not include value of output of the subsidized employment components as a benefit. Because the necessary disaggregations are not reported, comparisons with Supported Work's social costs are not possible, and comparison of overall net present value is clearly inappropriate. Nonetheless, a limited comparison with the WIN-II evaluation may be useful.

The most important comparison is with post-program earnings gains. Overall, WIN-II raises post-program earnings of females by a modest amount--$330 per year.[1] Schiller reports results for five different levels of services: no services, job search, education, training, and subsidized employment. The components most similar to Supported Work are OJT and PSE (classified as subsidized employment) and work experience (classified as job search). The annual net increases in earnings for females are $1,487 and $231 for subsidized employment and job search, respectively. Although differences in the length of follow-up, comparison group methodologies, and estimation techniques[2] make direct comparisons of the magnitudes of the earnings gains inappropriate, these results for WIN do serve to reinforce the conclusion from the Supported Work evaluation that job-creation programs appear to be able to raise the post-program earnings of female AFDC participants.

Earnings gains, of course, should not be judged without comparison to costs. Unfortunately, as noted above, the cost data reported by Schiller (1978) are federal WIN budget outlays, not social costs. These estimates cannot be compared with Supported Work's costs because they include participant wages and allowances instead of foregone earnings; they include the cost of child care and supportive services provided by WIN; they omit central administrative costs; and they omit some project and overhead costs.

---

[1] Unlike the estimates reported here for Supported Work, these and other estimates reported for WIN-II do not include fringe benefits.

[2] Work is in progress at Ketron which makes use of a longer follow-up period and different estimation techniques.

C. WILDCAT

The Wildcat Service Corporation, as discussed in Chapter I, was the first Supported Work program in the United States and the stimulus for the national Supported Work demonstration. A benefit-cost evaluation of the Wildcat program covering the first three years after enrollment was made by Vera (Friedman, 1978) between 1972 and 1975.[1] The program participants included in the study were ex-addicts, though Wildcat later enrolled ex-offenders and later still, when it became part of the national demonstration, youths and AFDC recipients.

Vera's (Friedman, 1978) findings are presented in Table V~~III~~.3 alongside the results for the national demonstration's ex-addict target group after 36 months.[2] Vera (Friedman, 1978) limited its benefit-cost analysis to the nonparticipant perspective. The necessary disaggregations were reported, however, so that benefits and costs from the participant and social perspectives can be determined.[3]

Both programs show a positive net present social value for three years. While the overall Wildcat findings are relatively more

---

[1] The results are reported by Lucy Friedman (1978), Chapter 14. A separate benefit-cost analysis of Wildcat was done by Lee Friedman (1977) based on preliminary data.

[2] Results for the national demonstration are based on measured differences for the first 27 months for the entire sample and the benchmark estimate for months 28-36; benefits were not extrapolated beyond 36 months.

[3] The nonparticipant benefit-cost findings are presented by Lucy Friedman (1978) in Table 14.1, p. 114; the needed earnings data are reported in Tables 9.4 and 9.5, pp. 65-67.

Table V.3

COMPARISON OF BENEFITS AND COSTS PER PARTICIPANT DURING THE FIRST THREE YEARS AFTER ENROLLMENT FOR WILDCAT AND EX-ADDICTS IN THE NATIONAL SUPPORTED WORK DEMONSTRATION

	Wildcat Service Corporation			National Demonstration		
	Perspective			Perspective		
	Social	Participant	Nonparticipant	Social	Participant	Nonparticipant
Benefits (dollars)						
Value of In-Program Output	12,834	0	12,834	3,363	0	3,363
Increased Post-Program Output (earnings)	1,536	1,536	-	-5	-5	0
Increased Tax Payments by Participants	0	-1,092	1,092	0	-317	317
Reduced Transfer Payments	0	-2,569	2,569	0	-402	402
Reduced Administrative Costs of Transfers	a/	a/	a/	30	0	30
Reduced Criminal Justice System Costs	773	0	773	1,732	0	1,732
Reduced Personal Injury and Property Damage	a/	a/	a/	299	0	299
Reduced Stolen Property	a/	a/	a/	180	-97	277
Reduced Drug/Alcohol Treatment Costs	a/	a/	a/	21	0	21
Reduced Use of Alternative Education and Employment Services	a/	a/	a/	79	0	79
Reduced Training Allowances	a/	a/	a/	0	-10	10
Costs (dollars)						
Program Operating Costs[b/]	-5,719	0	-5,719	-3,798	0	-3,798
In-Program Earnings	0	9,669	-9,669	0	3,777	-3,777
Foregone Earnings	-2,216	-2,216	0	-1,219	-1,219	0
Central Administrative Costs	a/	a/	a/	-201	0	-201
Net Present Value (Benefits Minus Costs)	7,208	5,328	1,880	480	1,727	-1,247

NOTE: The Wildcat results are the figures in Friedman (1978) inflated by 17.5 percent (the GNP deflator) to reflect prices in the fourth quarter of 1976, the middle quarter of the data collection period for the national Supported Work demonstration. The national Supported Work demonstration results are benchmark estimates for the 36 months after enrollment to make them as comparable as possible with the Wildcat estimates. Detail may not equal total due to rounding.

[a/] Not measured

[b/] Program operating cost is the sum of project and overhead costs.

favorable, the extent of the difference is misleading, and the overall comparison masks important differences in the results. Comparing some of the findings for individual benefit and costs components draws attention to some key differences between Wildcat and the national demonstration, as well as between Vera's (Friedman, 1978) evaluation techniques and those described in this report.

First, while Wildcat's program costs per participant are slightly higher than in the national demonstration, Wildcat's costs per year of service are over 40 percent lower.[1/] Several possible reasons for Wildcat's relatively lower operating costs can be identified. One of the causes is that Wildcat had relatively fewer materials-intensive construction and retail service work projects.[2/] Another likely explanation of Wildcat's lower operating cost is larger program scale. The Wildcat program at the time of Vera's (Friedman, 1978) study had about three times as many participants as the average site in the national demonstration, which would be expected to result in lower operating cost per participant.[3/]

Second, Wildcat's estimated value of in-program output is much higher than the benchmark estimate for the national demonstration. This

---

[1/] The average length of program stay by Wildcat participants was 75 weeks, compared to 29 weeks in the national demonstration. (Friedman, 1978, p. 113.)

[2/] Only 3 percent of the project slots listed in Appendix A to Friedman (1978) correspond to construction projects other than painting; none are associated with retail services.

[3/] Scale economies in Supported Work site operations are discussed in Kemper and Long, (forthcoming).

partly reflects differences in average length of stay. Even on a year-of-service basis, however, Wildcat's value of in-program output ($8,185) is 36 percent higher than Supported Work's ($6,018). Wildcat's higher estimated value of output alone exceeds social costs so that benefit-cost results are positive without including any additional benefits. One reason for the higher value of output is that more of Wildcat participants' enrollment time is spent working on projects, since Wildcat had less absence time and less program time in nonwork activities. Finally, although estimates of the value of output for Wildcat and the national demonstration were generally quite close for similar types of work, Wildcat emphasized activities that turned out to have higher value of output (net of project cost) than those emphasized by Supported Work. In addition, one exception to the generally similar value of output results was in an activity--building maintenance--that was important for both programs. The higher estimated value of output for building maintenance projects in the Vera (Friedman,1978) study further increased the Wildcat estimates relative to Supported Work.

Third, while the post-program earnings gain of participants in the national demonstration was estimated to be small, Wildcat participants registered a substantial gain. There is some evidence, however, that the post-program earnings of ex-addicts in the national demonstration may have improved over time in relation to the earnings of controls suggesting that post-program earnings gains may continue beyond three years. Post-program earnings gains for Wildcat, in contrast, decayed over the three-year period.[1]

---

[1] See Friedman (1978), especially pp. 67-71, 78-88.

Fourth, the differences in in-program earnings (due to Wildcat's longer average length of stay) and post-program earnings are reflected in the results for increased tax payments and reduced transfer payments. Despite certain measurement differences,[1] both Vera's (Friedman, 1978) evaluation and the present study estimated that approximately 12 percent of the total experimental-control earnings difference was paid in increased taxes. The greater reduction in transfer payments also mirrored the relative earnings results, though the increase in unemployment compensation to participants in the national demonstration--a transfer not measured in Vera's (Friedman, 1978) evaluation--also was a factor in this difference.

Finally, the benefits of reduced criminal activity for the national demonstration are substantially greater than those for Wildcat. This is due to both observed behavioral differences and differences in the techniques used to value behavioral changes. While Wildcat initially had a greater effect on participants in reducing overall arrests than did the national demonstration, the effect quickly decayed.[2] Vera (Friedman, 1978) valued the overall Wildcat arrest reduction, measured by interview data, only in terms of criminal justice system costs.[3] As described in Chapter III, arrest reductions resulting from participation in the national demonstration, also measured through interviews, were disaggregated by criminal charge and adjusted for respondent underreporting. The value attached to reductions of arrests took into account personal injury, property damage, and stolen property as well as criminal justice and insurance system costs.

---

[1] Vera estimated only sales and income taxes; the national demonstration evaluation also included social security taxes. Sales tax payments were imputed in a similar manner in the two studies, while income taxes were estimated somewhat differently. New York State and City sales and income tax rates for Vera (Friedman, 1978) were higher than in sites in the national demonstration.

[2] The arrest rate was much lower for Wildcat experimentals than controls during the first 12 months (.26 per participant year for experimentals, .58 for controls), about the same during the second 12 months (.31, .32), and higher during months 25-36 (.40, 127). Friedman (1978), p. 80.

[3] Friedman (1978), pp. 117-118.

The comparison thus indicates that the primary difference in results stems from differences in net cost (cost less value of output) apparently caused by differences in scale, time spent on output-producing activities, and work project mix. Both programs reduced arrests and increased post-program earnings, although the effects decayed rapidly for Wildcat but not for the national demonstration.

D. LIFE

LIFE (Living Insurance for Ex-Offenders) was a controlled experiment using random assignment conducted in Baltimore, Maryland, between 1972 and 1974. It provided financial assistance to a sample of males (randomly divided into experimental and control groups) who had been recently released from state prison and who had a high probability of recidivating.[1/] The program was designed to facilitate the job search activities of these ex-inmates and thereby ease their transition from prison into the labor market. LIFE thus differs from Supported

---

[1/] A component of LIFE provided job placement services in addition to or as an alternative to financial aid. However, this component did not appear to have any measurable effect on participant outcomes (Mallar and Thornton, 1978a). Therefore, evaluations of LIFE have focused on the financial aid component.

Work in two important respects. LIFE did not provide training or work experience, so that its scope is far more limited than that of Supported Work, and its target group differed from Supported Work's ex-offenders.

An evaluation of LIFE was conducted by Mallar and Thornton (1978a and 1978b) using procedures and assumptions similar to those used in the Supported Work evaluation. It estimated LIFE's social net present value per participant to be between $400 and $4,000, depending on what assumptions were made regarding displacement, discount rates, and decay rates.[1] The bulk of these benefits derive from increased output, with substantial reductions in burglary and larceny arrests also generating large benefits.

While the data from Supported Work do not allow a benchmark net present value estimate to be made for ex-offenders, a few comparisons can be made between the results for the first year in LIFE and the results from Supported Work for months 1-27. First, the reduction in total arrests was much larger for LIFE. The control-experimental difference in average number of burglary arrests in one year was 0.065 per participant for LIFE while the similar result for Supported Work during months 1-27 was -0.015 burglary arrests per participant per year (i.e., experimentals had more burglary arrests than controls). Given the estimated resource cost per arrest of burglaries, $8,749, this implies a substantial benefit for LIFE--total burglary benefits of $569 per participant for LIFE in one year--compared to an estimated net annual cost of $131 per participant for Supported Work ex-offenders. A comparison of other control-experimental differences for these groups of ex-offenders shows: (1) LIFE reduced larceny and motor vehicle theft arrests while there was an increase in these arrests for Supported Work; (2) in both programs there were estimated increases in robbery arrests, but that for LIFE was slightly larger than the increase observed for Supported Work; (3) the post-program earnings gain for LIFE was larger than that for Supported Work; and (4) changes in the use of transfer programs were similar.[1] Thus, the benefits for the first year after enrollment in LIFE appear to outweigh those for the first 27 months of Supported Work.

---

[1] The lower bound estimate assumed a 15 percent annual discount rate, a 14 percent annual decay rate for earnings and welfare differentials, and that half of participant's earnings gains reflected the displacement of nonparticipants. The upper bound assumed a 5 percent discount rate, no decay of earnings on welfare differentials, and no displacement.

On the cost side, the two programs also differ substantially. LIFE was a simple transfer program providing weekly cash payments for up to 13 weeks. As a result, estimated social costs were quite small, $125 per participant[2]. In comparison, the work project orientation of Supported Work led to greater costs, even when the value of output produced by work projects was subtracted as a partial offset to costs. Thus, the social investment in Supported Work was greater than that for LIFE. The social net present value of LIFE after one year was positive while that estimated for ex-offenders over the first 27 months of Supported Work was negative. Participants, however, were better-off financially in Supported Work because the transfers implicit in Supported Work wages exceeded the earnings and program payment gains achieved by LIFE participants. Beyond this early period, comparison of the programs is difficult because of the uncertainty over the benefits after month 27 for ex-offenders in Supported Work.

---

[1] The LIFE evaluation was not able to estimate changes in all the transfer programs considered in the Supported Work evaluation. Also, it did not estimate the value of changes in use of alternative employment and training programs or drug treatment programs. If these items were added to the LIFE evaluation, the results would not change substantially, since the change in alternative program use was quite small and the LIFE experimentals and controls were screened to eliminate persons with histories of drug or alcohol abuse.

[2] Administrative cost data were not available, costs were estimated on the basis of administrative costs incurred in public transfer programs.

Interpretation of results is also made difficult by the differences between the types of ex-offenders enrolled in the two programs--particularly LIFE's emphasis on repeat theft offenders with high a priori probabilities of recidivating. Because of this orientation it is not clear that LIFE's positive results would persist if the program were applied to a broader ex-offender population like that enrolled in Supported Work. Further evidence on this point comes from the evaluation of the Transitional Aid Research Project (TARP) (Rossi, Beck, Leniham, forthcoming), a program that did apply some of the LIFE program features to a general population of people released from state prisons in Georgia and Texas. An evaluation of TARP indicated no significant differences in criminality or earnings between experimentals and controls.

It can be concluded that LIFE did accomplish two of Supported Work's major goals--reducing criminal activity and increasing post-program earnings--at far less cost. It does not necessarily follow, however, that policymakers should favor it over Supported Work for a general population of ex-offenders. Increases in future benefits for Supported Work or preferences for Supported Work's goals--notably the transfer of income through wages paid for work--could make Supported Work more attractive.

E. JOB CORPS

Job Corps is a federal training program for disadvantaged youths that was established in 1964 and is now operated by the U.S. Department of Labor under Title IV of the Comprehensive Employment and Training Act (CETA). It differs from Supported Work in at least three important respects. First, it is largely a residential program. Second, participants spend most of their time in classroom education and training rather than in work activities. Third, while Job Corps provides a cash allowance to participants as well as support services, it does not pay them wages.

Results of an evaluation based on the first six months of post-program observation of Job Corps by Thornton, Long, and Mallar (1978) are presented in Table V.4 along with the results of the evaluation for Supported Work's youth target group. Overall, the findings for Job Corps are decidedly more favorable from the nonparticipant and social perspectives than those for Supported Work. Job Corps participants produce substantially more post-program output and show a far greater reduction in criminal activity. These major differences--along with relatively greater reductions in drug treatment costs, the use of alternative services,

TABLE V.4

COMPARISON OF BENEFITS AND COSTS PER PARTICIPANT FOR JOB CORPS AND YOUTH IN THE NATIONAL SUPPORTED WORK DEMONSTRATION

	Job Corps			National Demonstration		
	Perspective			Perspective		
	Social	Participant	Nonparticipant	Social	Participant	Nonparticipant
Benefits (dollars)						
Value of In-Program Output	757	86[a]	671	3,394	0	3,394
Increased Post-Program Output (earnings)	1,239	1,239	0	29	29	0
Increased Tax Payments by Participants	0	-107	107	0	-341	341
Reduced Transfer Payments	0	-1,011	1,011	0	-1,361	1,361
Reduced Administrative Costs of Transfers	120	0	120	328	0	228
Reduced Criminal Justice System Costs	1,896	0	1,896	853	0	853
Reduced Personal Injury and Property Damage	274	0	274	-1,356	0	-1,356
Reduced Stolen Property	387	-581	968	404	-218	622
Reduced Drug/Alcohol Treatment Costs	175	0	175	-116	0	-116
Reduced Use of Alternative Education and Employment Services	391	0	391	100	0	100
Reduced Training Allowances	0	-73	73	0	205	-205
Costs (dollars)						
Program Operating Costs[b]	-2,749	0	-2,749	-3,833	0	-3,833
In-Program Earnings	0[c]	1,384[c]	-1,384[c]	0	3,551	-3,551
Foregone Earnings	-879[d]	-726[d]	-153[d]	-974	-974	0
Central Administrative Costs	-1,359	0	-1,359	-203	0	-203
Net Present Value (Benefits Minus Costs)	251	39	212	-1,465	892	-2,357

NOTE: The Job Corps results are the benchmark figures in Thornton, Long, and Mallar (1978). The national Supported Work demonstration results are benchmark estimates. Detail may not equal total due to rounding.

[a] Some Job Corps work projects, such as on-center dormitory improvements, directly benefit corpsmembers.

[b] Program operating cost is the sum of project and overhead cost. For Job Corps, this also includes unbudgeted program costs estimated by Thornton, Long, and Mallar (1978).

[c] This includes corpsmember allowances plus the estimated value of in-kind transfers to Job Corps trainees in the form of room, board, and medical care. Corpsmembers do not receive in-program wages.

[d] Job Corps estimates of foregone earnings take into account "foregone" tax payments while in Job Corps. Post-program increases in tax payments are included as a separate benefit.

and training allowances--account for the overall differences. It also can be noted that, in this case, Supported Work constitutes the smaller of the two social investments: the net cost of Supported Work (program costs minus value of in-program output) is less than the net cost of Job Corps.

Despite their higher post-program earnings, corpsmembers do not come out as well from the participant perspective as participants in Supported Work. This is due to the fact that higher Supported Work in-program earnings dominate the participant perspective calculation.

Many of the differences for particular benefit and cost components mirror specific program differences. First, the difference in the value of in-program output results almost entirely from the fact that Job Corps participants spend most of their time in the classroom.[1] Second, supported worker wages and fringe benefits exceed the cash allowances and in-kind benefits provided to corpsmembers by design. Third, while the operating costs of the two programs are similar, a substantial part of the Job Corps expenditures is devoted to residential services and classroom staff, while a larger part of Supported Work's budget is devoted to inputs for work projects. Fourth, there is a highly positive benefit from reduced corpsmember criminal activity, while there is a slightly negative benefit for Supported Work associated with criminal behavior on the part of youth. The Job Corps effect on participant crimes, which was especially substantial during the in-program period, is probably due in part to that program's structured residential character, which takes youths who enrolled out of their home environment.[1/]

---

[1] On average, corpsmembers spend only 27 percent of their program days assigned to work activities; see Thornton, Long, and Mallar (1978), p. 35. By comparison, 82 percent of Supported Work hours are spent on work projects; see Kemper and Long (forthcoming).

Finally, it should be noted that, while these two evaluations are methodologically similar, there were three notable analytical differences. First, the Supported Work evaluation had an experimental design, while the Job Corps study used a carefully chosen comparison group of youths. Second, the Supported Work evaluation adjusted for respondent underreporting of arrests; since this was not done in the Job Corps study, the effect of Job Corps on criminal behavior is probably understated. Third, the Job Corps results are based on only one year of outcome measurement, and the benefit-cost results are sensitive to the assumptions made in extrapolating the post-program findings. An updated analysis, based on additional follow-up of corpsmembers, is scheduled for publication later this year.

F. CONCLUSION

The overview of benefit-cost analyses presented at the outset of this chapter enumerated the methodological inconsistencies that hamper comparisons like the ones attempted in this chapter. The most important lesson from this overview is that comparison of "bottom line" results--that is, looking only at net present value estimates for various programs--can be highly misleading. The specific program comparisons made later in

---

[1/] See Thornton, Long, and Mallar (1978), pp. 14-15.

the chapter were consequently limited to relatively similar evaluations of programs serving similar target groups, and were focused on findings for individual benefit and cost components rather than overall results.

At the same time, comparisons of benefit-cost results that go beyond the bottom line and examine the component benefits and costs can provide a very good picture of the relative merits and drawbacks of programs, of the differences in levels and types of social investments involved, of the amounts of income redistribution associated with the programs, and of which program effects and costs are important. For example, Job Corps' effect on participant crime is central to the last of the four comparisons presented. Highlighting that one benefit--discussing its measurement, comparing it to Supported Work's effect on criminal behavior, and identifying the role of program differences in determining the results--gives a valuable insight that would be lost in a quick recital of overall results.

Although important to arriving at policy judgments about the effectiveness of alternative programs, the state of much benefit-cost research is such that these useful component-by-component comparisons cannot be made. Questions about the credibility of measured outcomes because of the comparison-group methodology, differences in program accounting systems and cost definitions, and poor documentation of methodology and results often make comparison of specific components impossible. Even in those four cases where the program target groups and evaluation methodologies were relatively similar, we encountered difficulties in comparing the results once we began to delve more deeply.

The quality of benefit-cost evaluations generally, thus, needs to be improved if more useful comparisons are to be made. The evaluations must also be extended to a wider range of employment and training programs. The four studies included in the comparisons here evaluated only a fraction of the employment and training programs for the target groups served by Supported Work. This is especially true for the ex-offender and youth target groups. The primary alternative program for the AFDC target group is WIN, though there are other options such as the Work Equity Program (WEP) demonstration. There are few employment and training options for ex-addicts, though several drug treatment programs have training and placement components. There are, however, numerous alternative program approaches for ex-offenders and youth in addition to LIFE and Job Corps. Those for ex-offenders include prison training and prison work programs; work release, post-release training, and placement services and income support; and other work-experience programs for ex-offenders. Youth employment and training approaches, in addition to Job Corps, include a great many work experience and summer jobs programs under CETA.

Despite the limited number of comparisons made and the difficulties encountered, the four specific comparisons we did make provide useful insights. Although some qualifications, noted above, must be attached to the evaluation, the comparison to WIN-II reinforces the conclusion drawn from Supported Work that job programs can substantially raise post-program earnings of female AFDC recipients. The evaluations are too different, however, to determine whether one program is more effective in any overall sense than the other.

The comparison to Wildcat, although it shows somewhat different program effects for these two applications of the Supported Work concept, did find comparably positive overall results. This suggests that Supported Work is an effective employment and training approach for ex-addicts.

The LIFE results suggest that LIFE may be a socially efficient policy option for a special group of ex-offenders, but it is not clear that it would work if extended to the Supported Work target group. Indeed, there is evidence that other programs similar to LIFE, but for a broader targer group, have not achieved its level of success,[1] and other employment and training programs for ex-offenders have had mixed results at best.[2]

Finally, Job Corps--to which the Supported Work results for youth are compared--represents an unusual policy approach among employment and training programs.[3] Its largest benefit--reduced crime--appears to be partly due to its residential nature, which is not typical of employment and training programs. If the results of this preliminary evaluation of Job Corps are confirmed when the longer follow-up data are available, it will then appear as an efficient alternative to Supported Work. As noted above, however, Job Corps is not representative of programs for youth, and the narrowness of this comparison should thus be kept in mind.

---

[1] See the Employment and Training Report of the President (1979), pp. 199-200, Stephens and Sanders (1978), and Smith, Martinez and Harrison (1978) for discussion of the Transitional Aid Research Project (TARP).

[2] See Martinson (1975) for a review of the literature.

[3] Studies of other programs serving youths have mixed findings. See, for example, Borus, Brennan, and Rosen (1970) and Olympus Research Corporation (1971).

CHAPTER VI

CONCLUSIONS

This report has presented the results of an assessment of the benefits and costs of the national Supported Work demonstration. The purpose of this concluding chapter is to trace some of the implications of the results for employment and training policy and for future evaluation research.

A. POLICY IMPLICATIONS

Two sets of policy conclusions can be drawn from the benefit-cost evaluation of Supported Work. Overall judgments about the effectiveness of Supported Work for each of the four target groups obviously represent one important group of conclusions. Also important, however, are policy conclusions regarding individual benefit and cost components of the analysis that are key to the overall results.

1. Program Effectiveness

The results of the evaluation clearly indicate that Supported Work has been effective--from the perspective of society as a whole and the nonparticipants who bear the taxes required to fund the program--in providing services to AFDC recipients and ex-addicts. The overall results are positive for these two target groups, and only a drastic altering of the benchmark assumptions would change the overall conclusions.

The comparison of these results to those of similar programs serving similar target groups reinforces this conclusion. Preliminary results of an evaluation of WIN (the primary policy option for AFDC recipients) supports the conclusion that subsidized employment can be an effective policy for increasing the employability of AFDC recipients. An evaluation of Wildcat, an earlier application of the Supported Work concept in the U.S. and the only comparable program for this target group, presents additional evidence that Supported Work is an effective approach to the employment and crime problems of ex-addicts.[1/]

The evaluation results for the youth target group, however, are unambiguously negative. While participants are made better off by Supported Work, the results from society and nonparticipant perspectives are strongly negative--even under reasonable alternative assumptions. It is especially notable that virtually no benefits were observed for youths in the post-program period. These findings are in contrast to the available evidence for Job Corps, a comparable program for a youth target group, which found substantial reductions in crime, especially while enrolled in this residential program, and suggestive evidence in the immediate post-program period of an increase in post-program earnings.

Finally, the results from the evaluation of ex-offenders are unclear. The results for the first 27 months after enrollment are plainly negative--the least favorable among all four target groups. However, there is some evidence--based on a very small sample--of a reduction in arrests and an increase in earnings during months 28-36, making it extremely difficult to estimate future benefits. These future benefits, under some assumptions raise total benefits above costs from the standpoints of society and

---

[1/] The findings in the two evaluations did differ for individual benefit and cost components. For example, Wildcat's cost was lower and estimated value of in-program output higher (although this comparison should be made cautiously given that separate value of output estimates were not made for Supported Work.) The overall results, however, were highly favorable in both cases.

nonparticipants; under other assumptions, however, they do not. While we cannot say what future benefits will be, we can say that they would have to be very large for the overall ex-offender results to be as positive as for the AFDC and ex-addict groups.

These conclusions, as noted, are based on an evaluation of Supported Work from the perspectives of society as a whole and of nonparticipants, who pay the bulk of the taxes required to finance social programs. An assessment of the benefits and costs from the perspective of participants is also important, especially because Supported Work has intended distributional effects. The analysis showed that the demonstration has redistributed income to all groups except AFDC recipients, whose loss of welfare when they work is quite large. For this group, Supported Work wages and the increased post-program earnings--both of which are taxed--replaced a much more substantial amount of income from AFDC, Medicaid, food stamps, and other public transfer programs than was the case for the other target groups.

For all groups except ex-offenders, the results from all perspectives are remarkably insensitive to changes in the underlying assumptions. For the AFDC, ex-addict, and youth target groups, the qualitative conclusions are altered only when extreme, and we believe unrealistic, assumptions are made regarding benefits and costs. For ex-offenders, as we have noted, uncertainty about sensitivity of the magnitude of future benefits, and indeed whether they exist at all, to assumptions with respect to extrapolation makes it impossible to draw any firm conclusion. There are inevitably, as with all evaluations, further refinements that could be made in the methodology. First, regression models controlling for individual differences and incorporating corrections for interview nonresponse could be used to obtain more precise estimates of program effects. Although such refinements would undoubtedly affect the magnitude of estimates, however, they would almost certainly not affect the overall conclusions.[1] A second potentially important methdological refinement would be a more complete modelling of the decay of post-program benefits over time and the causes of differences among cohorts. Such models might make possible precise estimates of future benefits in general and might also help reduce somewhat the uncertainty surrounding future ex-offender crime benefits and future welfare reductions for AFDC recipients. Finally, analysis of the statistical significance of the benefit-cost results, similar to that done for ex-offender crime benefits, would be helpful in identifying areas of particular uncertainty. While all these refinements would be of research interest, and would enhance understanding of the results, again they would probably not change the overall qualitative conclusions drawn from the analysis.

2. Key Benefits and Costs

Four types of benefits and costs are especially important to the overall findings. The first of these is increased post-program earnings. Increased post-program earnings are a key objective of Supported Work and other employment and training programs.[2] In addition, the nonparticipant benefits of reduced transfers and increased taxes are a function of income and, hence, are closely associated with earnings gains. The benchmark estimates of post-program earnings differences varied dramatically across the four target groups in Supported Work. Increased post-program earnings--and consequently reduced transfers and increased taxes--dominated the benchmark results for the AFDC target group. However, post-program earnings differences turned out to be less important for ex-addicts, uncertain for ex-offenders, and negligible for youth. Given the important role of post-program earnings in determining overall results and the uncertainty about earnings differences beyond the period of measurement, it would be desirable to continue to monitor the earnings of the evaluation sample.[1]

---

[1] Estimates based on such procedures are presented in the outcome reports and do not differ substantially from the experimental-control mean differences used in this analysis.

[2] Increased earnings also are the one benefit consistently measured in employment and training program evaluations, and thus are an important element in program evaluations.

Another key benefit is reduced criminal activity. Reduced crime was the most important benefit for ex-addicts--so important in fact that the overall results are negative if the reduced crime benefit is excluded, but decidedly positive if crime benefits (nearly $4,200 per participant) are included. The results for youths, in contrast, show no evidence of a reduction in criminal activity, and the evidence for ex-offenders is ambiguous. Nonetheless, the crime reduction for ex-addicts, as well as the evidence for Wildcat and Job Corps, indicate that reduction in crime--because it is so costly to society--can be an important benefit of social programs, and suggest that employment and training programs may in some cases be able to reduce the criminal behavior of their participants.

Project cost--the costs of supervision, materials, equipment, etc. directly associated with operating the work projects on which participants gain work experience--and the value of project output, which are closely related to each other, also represent important components of the evaluation.

---

[1] Some further follow up of the AFDC target gtoup is currently planned, and all groups could be followed using social security records.

Project costs are the second largest component of costs, and project output is consistently one of the largest social benefits for all four target groups--only the benefits of post-program output of AFDC recipients and reduced criminal activity for ex-addicts overtake in-program output when estimated future benefits are added in. The substantial variation in how well projects were able to produce output of relatively high value (net of project cost) suggests that there may be potential for improving the performance of the program by modifying work project selection and operation.

Finally, overhead cost--the cost of enrolling participants, creating jobs, providing supportive services, placing participants in post-program jobs, and managing the program--represents the largest component of costs in the analysis. Finding ways of reducing it, without reducing program effectiveness in achieving its benefits, should be on the Supported Work policy agenda.

B. IMPLICATIONS FOR EVALUATION RESEARCH

This analysis has implications not only for the effectiveness of Supported Work as an employment and training policy, but also for various aspects of evaluation research. Among the many things that could be said about designing and carrying out benefit-cost analyses of social programs, the following seem to us to be especially important.

First, very careful consideration must be given to the fundamental issues in measuring program outcomes: the comparison group methdology, sample size, and length of follow-up. The concern here is not merely with the quality of evaluation research in general, about which all analysts must be genuinely concerned. As described in Chapter VIII,

441

comparison of evaluation results is made difficult when the evaluations diverge from the ideals of double blind random assignment to experimental and control groups, adequate sample size, and sufficient follow-up. Obviously, many factors must be weighed in designing program evaluations, including their costs, but the importance of the quality of the measurement of outcomes can hardly be overstated.

Second, a well-thought-out, consistent accounting framework is also invaluable to benefit-cost evaluations, especially those of complex social programs with multiple objectives. Such a framework should include not only the benefit and cost components, but also their impact from different perspectives. This allows users of the research to identify not only the large number of individual benefits and costs but also the distributional effects of the program. Equally important, it permits them to weigh the relative importance of various program and evaluation issues, and to assess interrelated program effects such as those of income, taxes, and public transfers. This applies not simply to the presentation of findings, but to the conduct of the evaluation itself.

Third, adequate attention from evaluators must be given to program costs. Frequently, expenditure data are received from program operators and used in an evaluation with much less thought than goes into analyzing program outcomes (i.e., potential benefits). As a result, "cost" can mean many things. It can mean the gross cost of the program, net cost (because certain expenses or the value of certain program outputs are excluded), "taxpayer" cost, ongoing costs, accrued cost or obligated cost. It is often limited to direct costs--that is, excluding unbudgeted costs, overhead, and ancillary costs--and sometimes refers only to administrative costs. There is an urgent need to standardize program cost concepts and to identify what costs should properly be measured from a given economic perspective--society, participants, nonparticipants, or any other that may be important for a given evaluation purpose.

Fourth, it is important that social program evaluations go beyond the traditional outcome measures: changes in post-program earnings and transfer program use. For example, the results for Supported Work ex-addicts indicate the need to include the value of crime related benefits. Because of the high cost of crime, even small changes in criminal activity can generate benefits that may outweigh those from increased earnings.[1/] The Supported Work results also show the importance of valuing in-program output. Net program costs would have been badly overstated if such an effort had not been made. In this evaluation these (and the other benefits and costs) have been valued on the basis of the market prices or resources used or saved and the outputs produced as a result of the demonstration. The limitations of this approach, particularly its failure to estimate social willingness to pay, are obvious, and refinements or superior alternative approaches would be valuable contributions to policy analysis; but the approach used here does allow these important benefits and costs to be valued and included directly in the benefit-cost calculations along with increased earnings.

Fifth, no matter how good the research design or the sample, evaluation results like the ones in this report are sensitive to assumptions made in extrapolating benefits and costs into the future. The problem is unavoidable because evaluation follow-up must be ended at some

---

[1/] A reduction of only 0.02 robbery arrests per participant per year would create benefits worth over $260 a year (0.02 arrests x $13,135/arrest). This value is of the same order of magnitude as the annual post-program earnings gains estimated for ex-addicts. Corresponding reductions in other types of crime would make total crime benefits greater than those for post-program earnings.

443

point and because policymakers need analyses as soon as possible. We have tried to make reasonable assumptions in making the benchmark extrapolation, and we have tested the sensitivity of the results to the assumptions. The fact that the results for ex-offenders were found to be so sensitive that we do not think it reasonable to make a benchmark estimate underscores the need for long-term follow-up data on a sample of substantial size. It also demonstrates the need for careful research on the methodology for estimating decay rates not only for earnings but for other effects as well.

The job of estimating the benefits and costs of the national demonstration is now finished, but the task of assessing Supported Work is not over. The accounting, the computations, and the dollar values reported in this analysis should not convey precision and finality--we certainly do not intend them to. The analysis is intended rather as a tool to be used, among others, in making judgments about the desirability of Supported Work. The purpose of benefit-cost analysis of social programs should not be to arrive mechanically at an estimate of net present value that dictates an investment decision. Rather, the function is to identify what issues and program effects are important to consider and to weigh the relative importance of these effects. Thus, for example, identifying what assumptions are most critical to the results may well provide more insight to a policymaker than assigning a value to a given benefit or cost. Readers who wish to make full use of the analysis, therefore, should recognize the sensitivity of the results to key assumptions, the relative roles of various benefit and cost components, and the potential importance of unmeasured effects and implementation difficulties.

REFERENCES

Ashenfelter, Orley. "Estimating the Effect of Training Programs on Earnings." Review of *Economics and Statistics*. 60(1978): 47-57.

Bjorklund, Peter B.; Schooley, F.A.; Byrd, W.; and Borgeson, N.S. *A Survey of Drug Abuse Treatment Costs*. Menlo Park, California: Stanford Research Institute, 1975.

Borus, Michael E.; Brennan, John P.; and Rosen, Sidney. "A Benefit-Cost Analysis of the Neighborhood Youth Corps: The Out-of-School Program in Indiana." *Journal of Human Resources*. 5(1970): 139-159.

Cain, Glen G. "Benefit/Cost Estimates for Job Corps." Institute for Research on Poverty Discussion Paper 9-67. Madison, Wisconsin: Institute for Research on Poverty, 1967.

Cain, Glen G. and Stromsdorfer, Ernst W. "An Economic Evaluation of Government Retraining Programs in West Virginia." *Retraining Unemployed*. Edited by Gerald G. Somers. Madison: University of Wisconsin Press, 1968.

"Cost-Benefit Analysis and the Job Corps." In *Assessments of the Job Corps Performance and Impacts*. Volume I. U.S. Department of Labor, Employment and Training Administration, Office of Youth Programs, 1979.

Dickinson, Katherine. "The Impact of Supported on Ex-addicts." Princeton, New Jersey: Mathematica Policy Research, Inc., 1979.

Drug Enforcement Administration. *Heroin-Related Crimes*. Washington, D.C.: Drug Enforcement Administration, February, 1977.

Employment and Training Report of the President. Washington, D.C.: U.S. Government Printing Office, 1979.

Friedman, Lee S. "An Interim Evaluation of the Supported Work Experiment." *Policy Analysis 3*, No. 2, Spring, 1977.

Friedman, Lucy. Vera Institute of Justice. *The Wildcat Experiment: An Early Test of Supported Work in Drug Abuse Rehabilitation*. Prepared as a Service Research Monograph of National Institute of Drug Abuse. (Washington, D.C.: U.S. Government Printing Office, 1978). DHEW Publication No. 79-782.

Griliches, Zin. "Earnings of Very Young Men." In *Income Distribution and Economic Inequality*. Edited by Zin Griliches, Wilhelm Krell, Hans-Jurgen Krupp, and Oldrich Kyn. New York: Halsted Press, 1978.

Haveman, Robert H. "The Dutch Social Employment Program." In Creating Jobs: Public Employment Programs and Wage Subsidies. Edited by John L. Palmer. (Washington, D.C.: The Brookings Institute, 1978).

Hertzman, M. and Montague, G. "Cost-Benefit Analysis and Alcoholism." Journal of Studies of Alcohol. 38(7), 1977.

Kemper, Peter. Supported Work Evaluation Supplementary Paper: Indirect Labor Market Effects in Benefit-Cost Analysis. Princeton, New Jersey: Mathematica Policy Research, Inc., forthcoming.

Kemper, Peter and Long, David A. "The Supported Work Evaluation: Technical Report on Value of In-Program Output and Costs." Princeton, New Jersey: Mathematica Policy Research, Inc., forthcoming.

Kemper, Peter and Moss, Philip. "Economic Efficiency of Public Employment Programs." Creating Jobs Public Employment Programs and Wage Subsidies. Edited by John L. Palmer. (Washington, D.C.: The Brookings Institution, 1978).

Lettre, Michael A. and Syntax, Anthony M. Application of JUSSIM to the Maryland Criminal Justice Planning Process. Towson, Maryland: Maryland Governor's Commission on Law Enforcement and the Administration of Justice, 1976.

Mallar, Charles D. and Thornton, Craig V.D. A Comparitive Evaluation of the Benefits and Costs from the LIFE Program. Washington, D.C.: American Bar Association, 1978a.

Mallar, Charles D. and Thornton, Craig V.D. "Transitional Aid for Released Prisoners: Evidence from the LIFE Experiment." Journal of Human Resources. XIII 2 (Spring, 1978b). 210-236.

Manpower Demonstration Research Corporation (MDRC). Summary of the Second Annual Report on the National Supported Work Demonstration. August, 1978.

Manpower Demonstration Research Corporation (MDRC). 1977 Report. (New York: 1978).

Martinson, Robert. "What Works?--Questions and Answers About Prison Reform." In Aldine Crime and Justice Annual for 1974, pp. 352-284. Edited by Seymour Halleck, et al. Chicago: Aldine Publishing Co., 1975.

Masters, Stanley. "The Impact of Supported Work on Long-Term Recipients of AFDC." Princeton, New Jersey: Mathematica Policy Research, Inc., 1979.

Maynard, Rebecca. "The Impact of Supported Work on Young School Dropouts." Princeton, New Jersey: Mathematica Policy Research, Inc., 1979.

Mincer, Jacob. Schooling, Experience, and Earnings. New York: National Bureau of Economic Research and Columbia University Press, 1974.

Nay, Jay N.; Scanlon, John W.; and Wholey, Joseph S. "Benefits and Costs of Manpower Training Programs: A Synthesis of Previous Studies with Reservations and Recommendations." In Joint Economic Committee Benefit-Cost Analyses of Federal Programs. pp. 249-274. Washington, D.C.: Joint Economic Committee, 92nd Congress, 2nd Session, 1973.

Olympus Research Corporation. "The Total Impact of Manpower Programs: A Four City Case Study." Report prepared for the Office of Policy, Evaluation, and Research, Manpower Administration, U.S. Department of Labor, 1971.

Organization of Economic Community and Development (OECD). Labour Market Policy in Sweden. Paris: 1963.

Piliavin, Irving and Gartner, Rosemary. "The Impact of Supported Work on Ex-Offenders." Princeton, New Jersey: Mathematica Policy Research, Inc. 1979.

Reubens, Beatrice G. The Hard Employ: European Programs. (New York, New York: Columbia University Press, 1970).

Rossi, Peter H.; Berk, R.A.; and Lenihan, Kenneth. Money, Work and Crime. New York Academic Press (forthcoming).

Schiller, Bradley R. "Lessons from WIN: A Manpower Evaluation." Journal of Human Resources. XIII 4(Fall, 1978): 502-523.

Schore, Jennifer; Maynard, Rebecca; and Piliavin, Irving. The Accuracy of Self-Recorded Arrest Data. Princeton, New Jersey: Mathematica Policy Research, Inc., May 15, 1979.

Sellin, Thursten and Wolfgang, Marving. The Measurement of Delinquency. New York: John Wiley, 1974.

Smith, C.L.; Martinez, P.; and Harrison, D. "An Assessment: The Impact of Providing Financial or Job Placement Assistance to Ex-Prisoners." Huntsville, Texas: Texas Department of Corrections, 1978.

Sourcebook of Criminal Justice Statistics 1977. Law Enforcement Assistance Administration, U.S. Department of Justice. Washington D.C.: U.S. Department of Justice, 1978.

Stephens, J.L. and Sanders, L.W. "Transitional Aid for Ex-Offenders: An Experimental Study." Atlanta: Georgia Department of Offender Rehabilitation, 1978.

Stromsdorfer, Ernst W. "Determinants of Economic Success in Retraining the Unemployed." Journal of Human Resources 3(1968): 139-158.

Stromsdorfer, Ernst W. Review and Synthesis of Cost-Effectiveness Studies of Vocational and Technical Education. Columbus: ERIC Clearing House on Vocational and Technical Education, 1972.

Thornton, Craig; Long, David; and Mallar, Charles. "A Comparative Evaluation of the Benefits and Costs of the Job Corps After Seven Months of Post-Program Follow-up." Technical Report D. (Princeton, New Jersey: Mathematica Policy Research, Inc., 1978).

☆U.S. GOVERNMENT PRINTING OFFICE: 1980-329-366/6607